IS PREGNANCY DIABETOGENIC?*

W. P. U. JACKSON
M.D. Cantab., F.R.C.P., D.C.H.
PHYSICIAN TO THE DIABETES CLINIC, GROOTE SCHUUR HOSPITAL, AND SENIOR LECTURER IN MEDICINE, UNIVERSITY OF CAPE TOWN, SOUTH AFRICA

IT is generally agreed that pregnancy often worsens the metabolic derangement of diabetes, increases the insulin requirement in juvenile type diabetics, and even produces a state of "temporary diabetes" in previously normal women who may return to apparent normality after delivery. In these "normal" women pregnancy is really uncovering a latent or prediabetic state, as is clearly shown by the rather remarkable case illustrated in fig. 1. This patient was seen in hospital at 8 months in her first pregnancy, in severe diabetic ketosis. She was shortly delivered, by cæsarean section, of a live 6 lb. baby. Between pregnancies her glucose-tolerance curve was normal, but the added "stress" of cortisone (50 mg. twice) revealed the underlying abnormality. Each pregnancy reaffirmed her basically diabetic status; and finally, because of a breast abcess, the hyperglycæmia was carried over into the puerperium. Since then she has remained "permanently" diabetic, even requiring 100 units of insulin per day.

I have also seen the vascular disorders of retinopathy and nephropathy either start or deteriorate during pregnancy, although this is uncommon, and the retinopathy may disappear after parturition (Becker 1952, Lawrence 1948, Oakley 1959).

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On all these grounds then, it has become usual to state that pregnancy is diabetogenic. But these considerations indicate a diabetogenic effect only in already established diabetics, or in prediabetics.

IS PREGNANCY DIABETOGENIC TO A NORMAL WOMAN?

Some workers have stated that, because tolerance for glucose is often impaired in completely normal women during pregnancy (reviewed by Cobley and Lancaster 1955), this indicates a "diabetogenic" effect of pregnancy in people with a basically normal metabolism. I do not subscribe to this, having found no evidence of impairment of glucose tolerance in *normal* pregnancy (excluding, as far as possible, all suspected prediabetics as well as known diabetics). In fact I have found the mean glucose-tolerance curve at the 36th week of pregnancy to lie *below* that of normal control women (Jackson 1952), and I have not found impairment of tolerance at other stages of pregnancy. This will be discussed in more detail elsewhere.

Hoet (1954) showed that pregnancy could potentiate a subliminal diabetogenic effect of cortisone or alloxan in rabbits, the combination causing a hyperglycæmia which neither agent could produce on its own. Similarly I have found (Jackson 1961) that the glucose-tolerance curve of normal pregnant women is often grossly abnormal if they have been primed with cortisone after the manner of Fajans and Conn (1954): thus the mean ordinary tolerance curve in pregnancy was not raised above that of the non-pregnant controls whereas the mean cortisone-augmented curve was distinctly higher than the mean cortisone curve in the control group (fig. 2*a* and *b*). In those subjects who

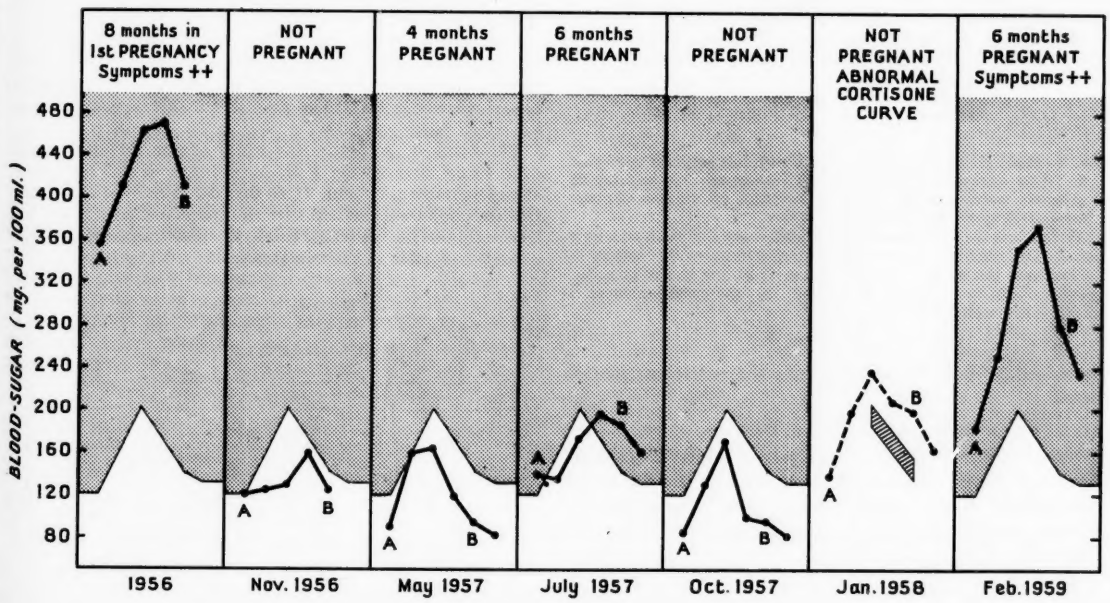

Fig. 1—To show the remarkable diabetogenic effect of pregnancy.
The patient was 22, and weighed 8 st. (112 lb.). The shaded zone is abnormal. A—fasting level; B—2 hours after 50 g. of glucose. Broken line—test after cortisone. The hatched zone marks the upper limits of "normality"; figures above it are abnormal.

were retested after childbirth, both ordinary and cortisone curves were normal (fig. 3).

This seems to mean that pregnancy puts a certain stress on a normal woman's pancreas (this is also shown by the well-known hypertrophy of the pancreatic islets), though no diabetogenic effect will be evident *unless* an additional factor, such as cortisone or the prediabetic state, is simultaneously present.

HAS THE DIABETOGENIC ACTION OF PREGNANCY ANY PERMANENT EFFECT?

The evidence so far adduced does not indicate whether the diabetogenic effect is temporary or permanent; nor whether the status quo is fully regained once pregnancy is completed (fig. 4).

The most important question is whether the diabetogenic effect of pregnancy can produce any lasting damage —i.e., whether pregnancy predisposes to clinical diabetes or its earlier appearance, or to more severe diabetic vascular disease. Apparently in favour of this are reports that the incidence of diabetes is higher in married women than single (Joslin et al. 1936, Mosenthal and Bolduan 1933), and that it rises with increasing parity (Pyke 1956, Fitzgerald et al. 1961). Let us examine this matter in greater detail.

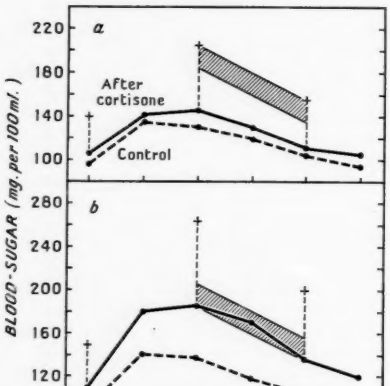

Fig. 2—Mean glucose-tolerance curves with and without cortisone in (a) 34 normal subjects under 45 (mean age 26); and (b) 40 pregnant women (mean age 26·5), including all months of pregnancy and up to the 5th postpartum day.

The crosses show 2 standard deviations above the mean cortisone curve. The hatching again marks the upper limits of "normality".

Note that the mean "control" curve is as low in the pregnant group as in the non-pregnant group, but that the cortisone curve is much higher, with a wider S.D.

Evidence from Parity and Incidence of Diabetes

Pyke (1956), by statistical analysis of evidence obtained from a diabetes clinic in Oxford reached the following conclusions, which have recently been confirmed by Malins and his coworkers in Birmingham (Fitzgerald 1961):

There was a great preponderance of women over men, but only over the age of 45.

Only a small part of this could be explained by greater obesity in women, and by the normal preponderance of women in the general population.

Parity afforded a complete explanation: nulliparous women had no greater prevalence of diabetes than men, while the incidence of diabetes in women rose with increasing parity until in the highest parity group it was about seven times commoner than in men.

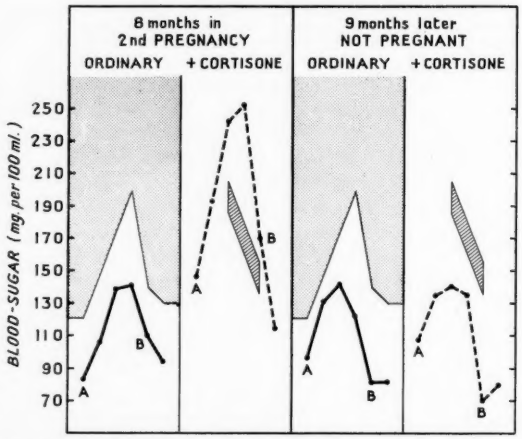

Fig. 3—An apparently "false positive" cortisone test during pregnancy.

The patient was 21, and weighed 10st. (140 lb.). She had had one previous child, who weighed 7 lb. at birth. There was no family history of diabetes, and she had no glycosuria except during the 2nd test. (Shading, hatching, A, and B as before).

Pyke concluded that "the effects of parity are alone enough to account for all the true differences in the sex-incidence of diabetes". I too set out to confirm these findings from the data available in our diabetes clinic, but I was unable to do so.

The following findings (based on Pyke's logical sequence) are from 869 randomly selected patients with enough data for analysis, from our clinic:

A preponderance of women, which starts at the early age of 35 (fig. 5).

Computed from the 1952 census figures (by correcting for female preponderance in the general population), the *expected* number of females diagnosed as having diabetes when over the age of 40 would be 239 as against our 200 males, whereas the *actual* figure was 508.

We obtained the figures for parity of non-diabetic women over 40 by interviewing a random sample of 782 outpatient attenders (table I, column 2), so that the same type of population supplied both this estimate and the diabetics. (Pyke used census figures for the whole country, which may not have been strictly applicable to his own area.)

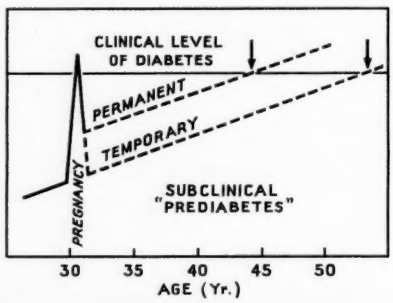

Fig. 4—The effect of pregnancy in prediabetes.

Pregnancy may reveal a "prediabetic state"; does it actually hasten the final development of clinical diabetes? Is the effect of pregnancy temporary or permanent?

The next step was to compute the *expected* number of diabetic women in each parity group (table I, column 3) from the expected total of women and the parity distribution in the normal series.

Finally we compared the observed with the expected numbers of diabetic women in each parity group. Table I (column 5) shows the proportion of observed over expected women in each group.

These findings indicate that:

The incidence of diabetes in women of all parity groups is considerably higher than the incidence in men.

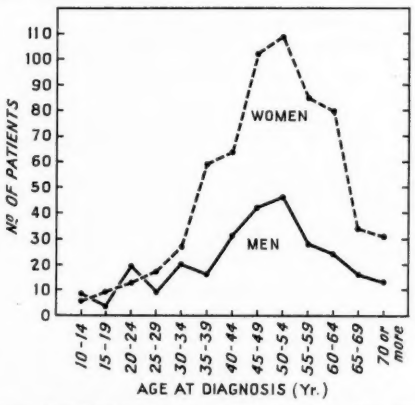

Fig. 5—Age-distribution by sex of 869 diabetics. The gross preponderance of women starts at 35.

There is a significant rise in the incidence of diabetes with increasing parity, *but* only over para-6.

Grande multiparæ (over ten full-time pregnancies) are common in this population, yet even in these the greater incidence of diabetes over that in nulliparæ is only just significant ($P=0.01$).

Parity alone cannot possibly explain the different sex-incidence of diabetes in our clinic.

The rising incidence of diabetes with high parity cannot be caused by increased obesity, because obesity was equally distributed among all parity groups in this series. I thought it might be explained by an *increased fecundity* of diabetics in their latent (prediabetic) stage. But Fitzgerald et al. (1961), working on a suggestion of McConnell (1956), found that diabetes in single women was no commoner than that in married nulliparæ.

The enormous preponderance of women attending our diabetes clinic may be due in part to three factors:

The sex ratio of the general population (as discussed above).

Greater obesity of the women (we classified 43% of our female diabetics as obese, against only 16% of males).

More diabetic men than women may not attend clinics at all, because of missing work, &c.; and these will not be included in series based on clinics.

TABLE I—PARITY AND INCIDENCE OF DIABETES

Parity	Normal women in each group (%)	Expected no. of diabetics in each group	Observed no. of diabetics in each group	Observed/Expected (%)
0	*9·9*	24	39	*156*
1	*13·2*	32	53	*160*
2	*16·4*	39	68	*165*
3	*11·1*	26	55	*196*
4	*11·8*	28	56	*187*
5	*8·2*	20	44	*210* (not significant*)
6	*7·8*	19	39	*205* (not significant)
7	*5·0*	12	36	*300* (P 0·05)
8–9	*7·7*	18	56	*310* (P 0·01)
10+	*8·9*	21	62	*295* (P 0·01)
Total	*100*	239	508	..

* As compared with the nulliparous group.

TABLE II—AGE AT DIAGNOSIS OF DIABETES IN WOMEN OVER 40 IN RELATION TO PARITY

Parity	No. of women	Average age at diagnosis (yr.)
0	40	52·0
1	58	53·0
2	75	52·5
3	57	50·7
4	62	53·0
5	45	54·5
6	46	52·5
7	97	51·7
8–9		
10+	68	52·8

But there are certainly other factors.

Parity and Age at Diagnosis

Pyke (1956) pointed out that, if repeated pregnancies increased the likelihood of diabetes, one would expect this disorder to appear at an earlier age in highly parous women. Yet he found that the age at diagnosis among the diabetics in his clinic was similar for all degrees of parity. My findings (which include many more highly parous women) are precisely the same (table II).

These combined findings weigh strongly against even repeated pregnancies having any permanent diabetogenic effect.

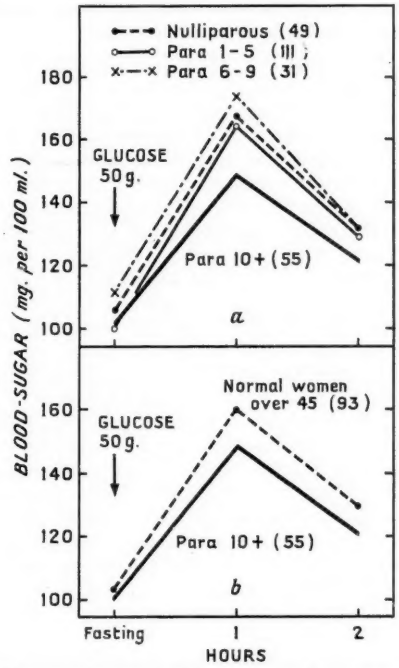

Fig. 6—Glucose tolerance in (a) various parity groups; and (b) grande multiparæ.

The grande multiparæ actually gave a lower mean tolerance curve than all other parity groups, and a lower mean curve than the other normal women over 45. (The figures in parentheses give the no. of subjects.)

Glucose Tolerance in Relation to Parity

Simple glucose-tolerance tests were arranged by Dr. F. Benjamin and carried out in the chemical pathology laboratories of Prof. J. Kench by methods identical with those used in our clinic.

246 women of various parity groups were studied. Most, but not all, were over 45. Known diabetics were excluded. Many had minor gynæcological complaints or post-

TABLE III—PARITY IN RELATION TO GLUCOSE TOLERANCE

Parity	No. of patients	Diabetic curves	Total abnormal curves
0	49	4 (8%)	15 (33%)
1–5	111	24 (22%)	34 (31%)
6–9	31	9 (29%)	13 (42%)
10+	55	5 (9%)	12 (22%)

menopausal bleeding. There is probably a high incidence of diabetes in women with abnormal endometria (Benjamin 1960), but these subjects were spread through all the parity groups.

Fig. 6*a* shows that there was no significant difference in the mean glucose tolerance of the different parity groups, except that the para-10+ group had a distinctly *lower* curve. The mean age of this group was lower (47 years) than that of the others (55, 56, and 57 years); but removing all the women under 45 makes no substantial difference to the curve (the mean figures become: fasting level, 98·1 mg. per 100 ml.; 1-hour level, 152·1 mg.; and 2-hour level, 125·1 mg.).

Fig. 6*b* shows separately the important para-10+ group, and the mean curve is compared with that obtained from 93 female controls, all of whom had a normal endometrium and were over the age of 45 (mean age, 53). The mean tolerance curve of these grande multiparæ is no higher than that of the normal group.

Table III indicates a tendency for the number of diabetic and "abnormal" curves to increase with parity, except that the para-10+ group contained *fewer* abnormalities, and was roughly on a par with the nulliparous group.

The similarity of the mean glucose tolerance in all parity groups argues against multiple pregnancies producing any lasting damage to pancreatic function. From evidence previously discussed—that the incidence of diabetes rises with parity—the tendency towards an increased number of abnormal curves with greater parity was to be expected. In fact the number of diabetic curves in the para-6–9 group is just possibly significantly higher ($P=0.05$) than that in the nulliparous group.

I expected to obtain the best evidence from the grande multiparæ. Murphy (1957) actually claimed that glucose tolerance was depressed in over half the women who had borne ten or more children; but his conclusions suffer from lack of a control series. Rather surprisingly, the 55 grande multiparæ in my series showed comparatively little impairment of glucose tolerance however they were analysed. Their mean tolerance seems better than that of other parity groups, probably because they included fewer women with abnormal endometria. At all events, I can obtain no evidence from them that repeated pregnancies reduce glucose tolerance.

Evidence from Retinopathy in Relation to Parity

260 women over 45, randomly selected from those attending our diabetes clinic, were examined by an ophthalmologist for evidence of retinopathy, and its severity. The results (table IV) show no significant difference in the incidence of retinopathy between the different parity groups; nor was any difference observed in the severity.

TABLE IV—INCIDENCE OF RETINOPATHY IN RELATION TO PARITY IN 260 DIABETIC WOMEN OVER 45

Parity	No retinopathy	With retinopathy	Total
0	20 (62·5%)	12 (37·5%)	32
1–5	82 (61%)	52 (39%)	134
6+	55 (58·5%)	39 (41·5%)	94

There was thus no evidence that parity is related to the incidence of retinopathy.

Conclusions

Pregnancy has a temporary "diabetogenic" effect in prediabetic women and in established diabetics.

There is no evidence that pregnancy is diabetogenic in normal women, except when combined with additional stress (e.g., cortisone).

Figures relating to parity, age at diagnosis, and incidence of diabetes, and also glucose-tolerance tests in different parity groups, show no good evidence that parity is important in producing overt diabetes.

From evidence obtained in a diabetes clinic, parity appears to play no part in the incidence or severity of retinopathy.

Certain discrepancies seem to be best resolved by assuming that the prediabetic is, statistically speaking, more fertile than normal.

All things considered, it is surprising to find no evidence in favour of the theory that pregnancy produces lasting diabetogenic effect. (The possible diabetogenic effect on the fœtus is not considered here.)

Appendix

Methods

Subjects for glucose-tolerance tests have a high carbohydrate intake for at least a week beforehand. 50 g. of glucose in 250 ml. of water is ingested after an overnight fast, and blood-sugars are estimated when fasting and at $^1/_2$-hourly intervals up to $2^1/_2$ hours. Capillary blood and Hagedorn's modification of the Hagedorn-Jensen method are used (Jackson 1952).

Interpretation

A glucose-tolerance curve is considered "normal" if the points are below 120 mg. per 100 ml. when fasting, 200 mg. at 1 hour, and 140 mg. at 2 hours. If two or three of these are exceeded, the curve is "diabetic"; if one only (usually the 2-hour level) is exceeded, it is "abnormal" (Jackson 1952). Blood-sugars are not usually performed in duplicate, but if a figure is obviously incorrect as judged by its relation to the figures next to it, it is discarded (e.g., 100 when fasting, 160, 170, 130, **200**, and 96 mg.). These normal standards are obtained from *young* people (Jackson 1961), and they are probably not directly transferable to those over 45; but this consideration does not alter the arguments in this paper.

Cortisone-augmented Glucose-tolerance Tests

For details see Jackson (1961).

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LONG-TERM EFFECT OF PRE-ECLAMPSIA ON BLOOD-PRESSURE

ELEANOR M. ADAMS
M.A. Cantab., M.D. Aberd., D.Obst.
MEDICAL RESEARCH COUNCIL RESEARCH OFFICER *

IAN MACGILLIVRAY
M.D. Glasg., F.R.C.O.G.
SENIOR LECTURER,
DEPARTMENT OF MIDWIFERY, UNIVERSITY OF ABERDEEN †

THOUGH much has been written on the subject, the later effects of pre-eclampsia are still uncertain. Difficulties arise because definitions and concepts are variable and often vague. Is pre-eclampsia the first expression of an underlying hypertensive tendency; or is it a cause of hypertension in middle age? Though pre-eclampsia and hypertension may overlap, should they perhaps be regarded as separate conditions, different in ætiology and prognosis? If so, how are they to be distinguished?

An investigation to answer questions such as these is hard to design. In the present study, which formed part of a larger investigation of blood-pressure in pregnancy (Adams 1960, MacGillivray 1961), the question asked was whether blood-pressure in middle-aged parous women depends on whether or not they have had pre-eclampsia. We decided also to compare the blood-pressures of parous middle-aged women with those of nulliparous women.

Subjects Investigated

Fairly detailed records of women delivered in the Aberdeen Maternity Hospital are available back to 1938. From the records for 1938–43, primiparæ then under 30 years of age were selected who, in 1958–59, were between 35 and 50. We would have preferred to select younger primiparæ and a narrower and later age-range at the time of follow-up, but we needed to obtain reasonably large numbers. Although the age-range at follow-up (35–50) is rather wide, the correlation between age and blood-pressure within this range is small ($r=0{\cdot}2$).

Suitable cases with an address in the city of Aberdeen were written to, twice if necessary. If no reply was received, inquiries were made at the last known address. By these means, about half the cases at risk were contacted. Very few then refused to cooperate.

Three groups of women were selected:

(1) 53 who had had "severe pre-eclampsia" in their first pregnancies. Severe pre-eclampsia was defined as a rise of the diastolic blood-pressure in the last trimester to 90 mm. Hg or more, on at least two occasions, together with definite proteinuria (0·25 g. per litre, or more). The blood-pressure fell to normal during the puerperium.

(2) 96 who had had "mild pre-eclampsia" in their first pregnancies. This was defined as a rise of diastolic pressure in the last trimester to 90 mm. Hg or more on at least two occasions, without proteinuria. The blood-pressure fell to normal during the puerperium.

(3) 185 whose diastolic pressures had remained below 90 mm. Hg throughout their first pregnancies and who had no proteinuria.

A fourth group consisted of 197 nulliparæ aged 35 to 50. Most of them were teachers, the others being factory workers and shop assistants, together with a few nurses. Most of the married nulliparæ had attended a clinic for the investigation of infertility, or had adopted a child. Some of those approached were found to have had an abortion, and were excluded. Single women were not asked whether they had had illegitimate pregnancies.

* Now in general practice in Derby.
† Now professor of obstetrics and gynæcology at St. Mary's Hospital Medical School, London, W.1.

Method

Nearly all the parous women were seen in hospital, by appointment; a few were seen at home. Most of the nulliparous women were seen at their places of work. All interviews and examinations were by one investigator (E. M. A.) except for 21 nulliparous factory workers who were seen by I. MacG.

First, weight and height were measured in indoor clothes, without shoes.

For the next ten minutes, subjects rested lying down or sitting in a chair, and during this time questions were asked about general health, menstrual history, family history, and, where appropriate, pregnancies.

Finally, blood-pressure was measured on the right arm, by the technique recommended by the Cardiac Society and the American Heart Association (1939). After the subject had rested for at least ten minutes, the sphygmomanometer cuff was applied smoothly and firmly with the middle of the bag approximately over the brachial artery. The cuff was inflated to about 30 mm. Hg above the level at which the radial pulse ceased. The systolic pressure was then recorded to the nearest 2 mm. Hg and the diastolic pressure was recorded at the point of muffling of the sounds.

Arm-girth measurements were made but are not reported here. Variations in arm-girth account for less than 25% of the difference between direct and indirect measurements of blood-pressure, and for a given arm circumference this difference varies widely (Pickering et al. 1954). It was concluded that correction of the recorded pressures according to arm-girth would make so little difference to the results that the extra work of calculation was not worth doing.

Results

Table I gives the average characteristics of the four groups, and their average blood-pressures.

The highest pressures, both systolic and diastolic, were found in the women whose first pregnancies had been complicated by *mild* pre-eclampsia. Their average blood-pressure was significantly higher than that of the women who had had severe pre-eclampsia, and these in turn had significantly higher average pressures than parous women whose first pregnancies had been normotensive. The average blood-pressures of the nulliparous women were slightly higher than those of the "normotensive" parous women (group 3); the difference is not significant for diastolic pressure, and almost significant for systolic pressure. Breakdown of the nulliparæ into the married and unmarried showed the married to have a significantly higher average systolic pressure than group 3.

These differences of blood-pressure cannot be attributed to differences in physical characteristics of the groups.

TABLE I—AVERAGE PERSONAL CHARACTERISTICS AND BLOOD-PRESSURES OF FOUR GROUPS OF WOMEN AGED 35–50 YEARS

(Groups 1, 2, and 3 are defined according to the nature of their first pregnancies. Numbers in parentheses are standard errors of the means.)

—	Group 1 (severe pre-eclampsia)	Group 2 (mild pre-eclampsia)	Group 3 (no pre-eclampsia)	Group 4 (nulliparæ)
Number of subjects	53	96	185	197
Weight (lb.)	139·9 (3·0)	144·0 (2·7)	137·3 (1·9)	135·9 (1·5)
Height (in.)	61·9 (0·25)	62·0 (0·21)	61·8 (0·16)	62·8 (0·18)
Age (years)	42·8 (0·5)	41·6 (0·4)	42·0 (0·3)	45·0 (0·2)
Interval since delivery of first child (years)	16·9 (0·4)	17·6 (0·2)	17·8 (0·8)	.. ..
Systolic pressure (mm. Hg)	138 (2·3)	147 (2·3)	132 (1·3)	137 (1·4)
Diastolic pressure (mm. Hg)	89 (1·3)	93 (1·3)	84 (0·7)	86 (0·8)

TABLE II—PERCENTAGE INCIDENCES OF "HYPERTENSION" IN EACH GROUP

Group	Systolic pressure 140 mm. Hg or over %	Diastolic pressure 90 mm. Hg or over %
1. Severe pre-eclamptics	43	40
2. Mild pre-eclamptics	58	60
3. Non-pre-eclamptics	26	21
4. Nulliparæ	41	35

The mild pre-eclamptics (group 2) were slightly heavier than the others, and the nulliparæ (group 4) were slightly older, taller, and lighter; but none of these differences is significant.‡

Table II gives the percentage of women in each group having "hypertension", according to lower limits frequently used (i.e., 140 mm. Hg systolic, and 90 mm. diastolic. As suggested by the means in table I, women in group 2 (mild pre-eclampsia) had the highest percentages. Group 1 (severe pre-eclampsia) and group 4 (nulliparæ) were closely similar, with considerably more "hypertension" than group 3 (no pre-eclampsia).

The figures in tables I and II are not significantly altered when women in each group are divided into premenopausal and postmenopausal categories.

The influence of pregnancies after the first was also investigated. The average parity in groups 1, 2, and 3 was similar. Since not all pregnancies after the first occurred in hospital, the records are less reliable; but it is possible to make a reasonable classification of these later pregnancies into "hypertensive" and "normotensive", using the criteria adopted for groups 2 and 3.

Table III gives the mean systolic and diastolic pressures in groups 1, 2, and 3, according to whether there was no

TABLE III—MEAN SYSTOLIC AND DIASTOLIC BLOOD-PRESSURES (mm. Hg) AT FOLLOW-UP RELATED TO PRE-ECLAMPSIA IN FIRST PREGNANCY AND HYPERTENSION IN SUBSEQUENT PREGNANCIES

(Systolic blood-pressures on the left, diastolic on the right. Numbers in parentheses represent numbers in subgroups.)

Hypertension in subsequent pregnancies	Pre-eclampsia in first pregnancy					
	Severe (group 1)	Mild (group 2)	None (group 3)	Severe (group 1)	Mild (group 2)	None (group 3)
No subsequent pregnancies ..	142	150	135	90 (9)	94 (20)	83 (23)
No hypertension ..	133	139	129	87 (10)	88 (26)	83 (101)
Hypertension ..	138	149	136	89 (34)	96 (50)	86 (61)

pregnancy after the first, or a normotensive pregnancy, or a hypertensive pregnancy. In all three groups, the lowest averages are in those women who had normotensive subsequent pregnancies only. There is little difference between the women who had no subsequent pregnancy and those who had a hypertensive subsequent pregnancy.

Table IV consolidates the figures for parous women according to whether hypertension was experienced in one pregnancy only, in more than one pregnancy, or not in any pregnancy. As might be expected, the lowest average blood-pressures were found where hypertension was never recorded in any pregnancy, and highest in women who had hypertension in more than one pregnancy.

‡ The coefficients of regression of blood-pressure on height and weight do not differ significantly between the groups; that is, change of blood-pressure with height and with weight is similar in all groups. Taking all groups together, the within-group coefficients are 0·099 for weight and −0·62 for height for diastolic pressure, and 0·16 for weight and −1·05 for height for systolic pressure. A significant part of the variance of both systolic and diastolic pressure is "explained" by the regression on weight and height, but almost all the "explained" variance is contributed by weight.

Discussion

For obvious reasons, it is difficult to discover the eventual aftermath of pre-eclampsia in a first pregnancy, and no completely satisfactory controlled study has been published. The difficulties are illustrated in the report of Gibson and Platt (1959) who re-examined cases of pre-eclampsia observed by Gibson (1956) in Belfast, and compared their blood-pressures with those of a sample of London women whose blood-pressures were determined by Hamilton et al. (1954). The proportion of women in the control group who had had normal or toxæmic pregnancies is unknown, and Gibson and Platt comment that "the ideal control would have been a series of women who had passed through a non-toxæmic pregnancy".

TABLE IV—MEAN SYSTOLIC AND DIASTOLIC PRESSURES RELATED TO HYPERTENSION IN ANY PREGNANCY

All pregnancies	No.	Mean systolic pressure (mm. Hg)	Mean diastolic pressure (mm. Hg)
No hypertension	124	131	84
Hypertension in one pregnancy	125	139	87
Hypertension in more than one pregnancy	85	145	93

The fact that fairly reliable obstetric records are available in Aberdeen back to 1938, and that the population of the city is fairly stable, has enabled some of the difficulties to be overcome. Though only about half the cases at risk were identified and examined at follow-up, we have no reason to suspect that an important bias has been introduced by the losses.

Our data indicate that among middle-aged parous women blood-pressure tends to be higher in those who had pre-eclamptic first pregnancies. This effect was greater after mild than after severe pre-eclampsia. Furthermore, the prevalence of hypertension was similar in middle-aged nulliparous women and in parous women who had had severe pre-eclampsia; in both these groups it was higher than in parous women whose pregnancy had not been complicated by pre-eclampsia, and lower than in parous women who had had mild pre-eclampsia.

To explain these findings, it may be postulated that the term "mild pre-eclampsia", applied to a slight rise of blood-pressure in late pregnancy not complicated by other evidence of abnormality such as proteinuria, is sometimes a misnomer. In other words, "mild pre-eclampsia" is not always a less serious form of "severe pre-eclampsia", but is something different.

There are several pieces of evidence that they are two different, though overlapping, conditions. The incidence of "mild pre-eclampsia", but not of "severe pre-eclampsia", shows a sudden rise in primigravidæ above the age of 30 (Nelson 1955). Birth-weight is not depressed (in comparison with the average for the period of gestation) in "mild pre-eclampsia", but it is sometimes depressed in "severe pre-eclampsia" when this condition occurs well before term (Baird, Thomson, and Billewicz 1957). Primigravidæ who develop "severe pre-eclampsia" are, on the average, not appreciably heavier than normal in early pregnancy, but they gain weight unusually fast, especially in late pregnancy; by contrast, primigravidæ who become hypertensive tend to be heavier than normal in early pregnancy but do not gain weight unusually fast (Thomson and Billewicz 1957). It now appears that "mild

pre-eclampsia " has a greater tendency to be followed by hypertension in middle age than " severe pre-eclampsia ".

It may be supposed that certain women have an underlying hypertensive trait which will become permanently manifest in later life. This tendency may be temporarily unmasked by pregnancy—especially by a first pregnancy. In such pregnancies, pre-eclampsia (hypertension with proteinuria) may or may not be superimposed. The hypertensive tendency doubtless increases the risk of a superimposed pre-eclampsia, but even " normotensive " women are exposed to the risk of pre-eclampsia.

On this basis, group 1 in this study consisted predominantly of women with an underlying hypertensive tendency. Group 2, the women with severe pre-eclampsia, were a mixture of those with and without such a tendency and so their blood-pressures in middle life were lower. Group 4, the nulliparous women, were also a mixture of women with and without a hypertensive tendency and their blood-pressures were comparable with those in group 2. On the other hand, group 3, whose first pregnancies had not been complicated by hypertension, were predominantly women without an underlying hypertensive trait and so, in middle age, they had the lowest average blood-pressures of all the groups.

This explanation is based on the evidence of the first pregnancies in parous women, but it is further supported by considering subsequent pregnancies in conjunction with the first. When subsequent pregnancies were normotensive, average blood-pressures in middle age were lower than when subsequent pregnancies were hypertensive. The difference was greater when pre-eclampsia in the first pregnancy was mild than when it was severe.

These findings, and the interpretation placed upon them, appear to be consistent with the opinions of Browne and Dodds (1939), Bryans and Torpin (1949), and Dieckmann (1952) that women with a hypertensive tendency are more liable to have a rise of blood-pressure during pregnancy, and that they would ultimately become hypertensive even if they never became pregnant. Pre-eclampsia, per se, does not predispose to hypertension in middle life; but women who do have pre-eclamptic pregnancies include a greater-than-average proportion of those with the hypertensive trait.

Summary

At least fifteen years after their first pregnancy, the blood-pressure was tested in 149 women who had had pre-eclampsia during that pregnancy and in 185 women who had been normotensive. In addition 197 nulliparous women were studied. All the women were between 35 and 50 years of age when examined.

The mean blood-pressures (systolic and diastolic) of those who had had mild pre-eclampsia or severe pre-eclampsia were significantly higher than the mean blood-pressures of those who had not had pre-eclampsia. The nulliparous women had a higher mean blood-pressure, and a greater proportion of higher blood-pressures, than the parous women who had not had pre-eclampsia.

The findings support the view that pre-eclampsia is not a direct cause of hypertension in later life. In some women diagnosed as having mild pre-eclampsia the rise of blood-pressure may represent the temporary unmasking during pregnancy of a hypertensive tendency which subsides after the delivery but returns in later life. The proportion of women with severe pre-eclampsia who have this hypertensive tendency is no greater than average.

The helpful advice and constructive criticism of Sir Dugald Baird, Dr. A. M. Thomson, and other colleagues in the Obstetric Medicine Research Unit during this investigation and in the preparation of this paper are gratefully acknowledged.

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FAMILIAL ASPECTS OF PRE-ECLAMPSIA AND HYPERTENSION IN PREGNANCY

ELEANOR M. ADAMS
M.A. Cantab., M.D. Aberd., D.Obst.
MEDICAL RESEARCH COUNCIL RESEARCH OFFICER *

ANGELA FINLAYSON
B.A. Lond.
SOCIOLOGIST

OBSTETRIC MEDICINE RESEARCH UNIT
MEDICAL RESEARCH COUNCIL, UNIVERSITY OF ABERDEEN

FEW studies of the familial aspects of pre-eclampsia and hypertension in pregnancy have been published, and their results are difficult to evaluate. The composition of the population studied and the criteria adopted have not always been made clear.

Barnes and Browne (1945) measured the blood-pressures of 106 first-degree relatives of 70 patients with pre-eclampsia and of 66 first-degree relatives of 47 patients with no pre-eclampsia or hypertension. The parity and age of the patients are not stated, and pre-eclampsia was diagnosed if the blood-pressure exceeded 120/80 mm. Hg after the 20th week of pregnancy—a definition that would include many women whom other workers would not regard as having pre-eclampsia (or hypertension). 46% of the relatives of the pre-eclamptics were found to be hypertensive and 39% of the relatives of the normotensives.

At an unspecified time after delivery, Browne and Sheumack (1956) measured the blood-pressures of 100 primigravidæ who had had pre-eclampsia, and also the blood-pressures of 72·7% of 433 of their relatives. (Whether the relatives were of first degree is not stated). 12% of the pre-eclamptic primigravidæ had a " residual " hypertension of over 140/90 mm. Hg. Of those who had at least one hypertensive relative 14·7% (10) had hypertension, while among those with no hypertensive relative, 6·2% (2) had hypertension. In these last 2 cases, both parents of one died from cardiovascular disease and the father of the other died after a coronary occlusion, while the mother's death was attributed to gallbladder disease; hypertension might therefore have been found in both families, had it been possible to measure the blood-pressures of these parents. These results are suggestive, but the numbers are small.

In testing a hypothesis that pre-eclampsia is caused by serological incompatibility, Platt, Stewart, and Emery (1958) asked medical graduates for information about pre-eclampsia in their near female relatives, by blood or marriage. They assumed that such information would be reliable, which might be so in the case of the doctors' wives or the women doctors themselves; but in respect of the remaining relatives this seems an unwarranted assumption. They did not find evidence of serological incompatibility, but they noted that the sisters of toxæmic women appeared unusually liable to toxæmia.

Kaku and Nagata (1959) reported that women from families

*Now in general practice in Derby.

with a " hypertensive lineage " had more pregnancy toxæmia than others. The precise meaning of a " hypertensive lineage ", however, is not given; nor is pregnancy toxæmia defined. Some of the women were delivered at home (the proportion was not stated) and were regarded as having toxæmia if they had marked œdema of the legs or generalised œdema. There is no breakdown by age or parity.

Humphries (1960) compared 100 probands who had had hypertensive toxæmia of pregnancy with 200 controls who had had normal pregnancies, and found that 28% of the probands had themselves been born of a toxæmic pregnancy, as against 13% of the controls. The difference is significant ($r < 0.01$). On primiparæ only, the results are 28·7% as against 14·3%. Humphries' follow-up study of the mothers ten and twenty years after the index delivery showed that a raised blood-pressure was commoner among the mothers of probands than among the mothers of controls; but the difference was not statistically significant. He concluded that hypertensive toxæmia of pregnancy displays " familial aggregation " and that it is one expression of a familial (presumably inherited) tendency towards hypertensive disease.

We felt that there was a need for further research, using defined populations and standard criteria, and that among the subjects requiring attention were:

(*a*) Investigation of the history of pre-eclampsia and hypertension in the sisters of women with pre-eclampsia or hypertension in pregnancy, compared with the sisters of normotensives. Since these complications occur more often in first pregnancies than in second or third, it seemed advisable to restrict study to primigravidæ.

(*b*) More systematic investigation of the blood-pressures of the mothers of women with pre-eclampsia or hypertension in pregnancy, compared with those of mothers of normotensives.

(*c*) Investigation of the blood-pressures of siblings of women, now middle-aged, who had had pre-eclampsia or hypertension in their first pregnancies, compared with those of siblings of middle-aged women who had been normotensive in their first pregnancies.

The three studies described below were designed to provide data on these subjects.

Methods

Primigravidæ delivered in hospital in Aberdeen are classified according to whether they remain normotensive throughout, or develop pre-eclampsia, or develop hypertension not classifiable as pre-eclampsia.

Three studies were made, each drawing index cases from the population just described and each using different categories of relatives for comparison.

The presence or absence of severe or mild pre-eclampsia was determined throughout by the criteria used by Nelson (1955). He described pre-eclampsia as a condition in which a rise of diastolic blood-pressure to 90 mm. Hg or higher either (1) occurs after the end of the 26th week of pregnancy, on two occasions separated by at least one day, or (2) occurs in labour and shows a progressive pattern. In the " mild " form there is no albuminuria, unless of a trivial or transitory kind; but in " severe " pre-eclampsia there is albuminuria exceeding 0·25 g. per litre (Esbach). Because of the difficulty in applying rigid criteria when examining records, œdema was not used in diagnosis or grading.

Results

Pre-eclampsia among Sisters of Pre-eclamptics

From the population of primigravidæ 419 index cases were obtained, each with one or more sisters whose first-birth record was available in Aberdeen.

Identification material on the sisters of 301 index cases was already available from a sociological study of families undertaken in 1957–58. Similar material on the sisters of the other 118 was obtained either by interview in the antenatal or lying-in wards during 1958–60 or, for patients delivered in previous years, by post. The hospital records of the sisters thus identified were then examined. After exclusion of cases where the pregnancy had been preceded by an abortion, the grading for pre-eclampsia or hypertension was extracted.†

TABLE I—INCIDENCE OF PRE-ECLAMPSIA IN THE FIRST PREGNANCIES OF SISTERS OF PRIMIGRAVIDÆ WITH AND WITHOUT PRE-ECLAMPSIA

Sisters	Index cases		
	No pre-eclampsia (273)	Hypertension (99)	Severe pre-eclampsia (47)
No pre-eclampsia ..	272 (*71·8%*)	60 (*51·4%*)	30 (*51·7%*)
Hypertension ..	90 (*23·8%*)	47 (*40·2%*)	20 (*34·4%*)
Severe pre-eclampsia	17 (*4·5%*)	11 (*9·4%*)	8 (*13·8%*)
Total	379 100	118 100	58 100

The results are set out in table I. Since 95% of all primigravidæ living in Aberdeen are delivered in hospital, we thought that restriction of the sisters to those with hospital records would not weight the sample unduly with pre-eclampsia. The validity of this decision seemed to be confirmed by the number of cases in which the sisters of normotensive index cases had had hypertension or severe pre-eclampsia in their first pregnancies. The proportion who had had hypertension (23·8%) was slightly higher than in the general primigravid population in Aberdeen, while the proportion who had had severe pre-eclampsia (4·5%) was slightly lower; but both are reasonably consistent with general experience.

In contrast, about 49% of the sisters of women with pre-eclampsia or hypertension had themselves had one of these conditions; which suggests a familial tendency to the disease. It is also noteworthy that sisters of hypertensives had a higher incidence of hypertension (40·2%) and a lower incidence of severe pre-eclampsia (9·4%) than the sisters of severe pre-eclamptics, for whom the figures are 34·4% and 13·8%. This may suggest that sisters tend to have pre-eclampsia of a similar degree of severity; but another explanation is possible—namely, that pre-eclampsia and hypertension of pregnancy are independent conditions, the latter being purely a manifestation of a hypertensive tendency.

TABLE II—MEAN SYSTOLIC AND DIASTOLIC PRESSURES (mm. Hg) OF MOTHERS OF PRIMIGRAVIDÆ WHO HAD HYPERTENSION OR NO HYPERTENSION IN PREGNANCY

First pregnancies of daughters	Mothers		
	No.	Systolic pressure	Diastolic pressure
None had hypertension ..	149	143 (S.D., 24·2)	90 (S.D., 12·4)
At least one had hypertension	87	151 (S.D., 22·8)	94 (S.D., 11·7)

Blood-pressure of Mothers of Pre-eclamptics

The blood-pressures of the mothers of primigravidæ were measured when they visited their daughters in hospital. An attempt was made to measure the blood-

† In a small proportion, both of index cases and sisters, the conditions for pre-eclampsia were not strictly fulfilled (e.g., a diastolic pressure of 90 mm. Hg or more occurring once only during the pregnancy or once before the 26th week and once afterwards) and yet these cases could not be said to be normotensive throughout pregnancy. They were too few to form a separate group; so, although such a procedure is not entirely satisfactory, they were added to the group of mild pre-eclamptics and all labelled " hypertensives ". Hypertension therefore, in this section, does not mean " essential hypertension ".

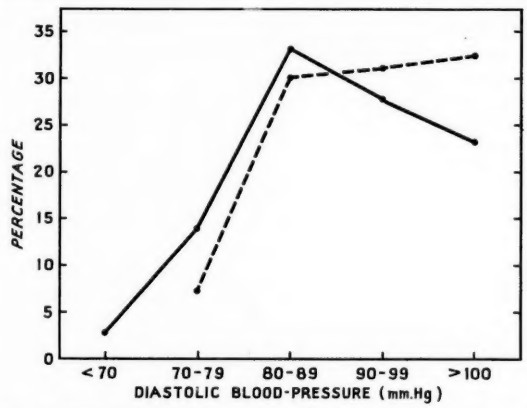

Fig. 1—Systolic pressure of mothers of pre-eclamptics and normotensives.

——— No daughter had hypertension in first pregnancy.
– – – – At least one daughter had hypertension in first pregnancy.

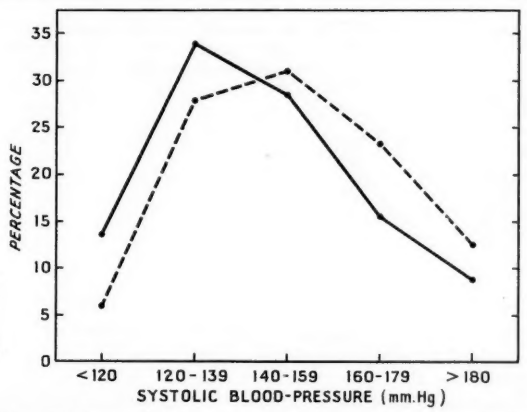

Fig. 2—Diastolic pressure of mothers of pre-eclamptics and normotensives.

——— No daughter had hypertension in first pregnancy.
– – – – At least one daughter had hypertension in first pregnancy.

pressures of the fathers also, but very few of them came. The nursing staff (sisters or staff midwives) measured some of these blood-pressures, since one person could not take all of them in the time for which the mothers were prepared to wait.

By these means, the blood-pressures of 236 mothers were obtained. In some instances, information was available about the first pregnancies of other daughters as well. Accordingly the mean systolic and diastolic pressures have been calculated according to whether (*a*) no daughter had any hypertension in a first pregnancy, (*b*) at least one daughter had hypertension in a first pregnancy, or (*c*) more than one daughter had hypertension. Unavoidably, information about many of these families is incomplete. Some daughters are still childless and others have had their first babies at home or away from Aberdeen.

Systolic and diastolic means for the mothers are given in table II. The means for the groups with one or more than one daughter with hypertension in first pregnancy (150/93 mm. Hg against 153/94) are not significantly different; but, if these groups are pooled, the pooled means are significantly higher than the means for mothers whose daughters had no hypertension as primigravidæ. Unfortunately, as the ages of most of the mothers were not obtained, no correction can be made for age; but the age distributions of the daughters in the three groups are rather similar, so it is very likely that the age distributions of the mothers are also similar. The frequency curves of systolic and diastolic pressures (figs. 1 and 2) are shifted to the right for the mothers who have at least one daughter with hypertension in her first pregnancy, as compared with those for the mothers of daughters who had no hypertension.

TABLE III—MEAN SYSTOLIC PRESSURES (mm. Hg) OF SIBLINGS ACCORDING TO AGE AND BEHAVIOUR OF INDEX CASE IN FIRST PREGNANCY

Siblings' age (yr.)	Index case (first pregnancy)			
	Mild pre-eclampsia	Severe pre-eclampsia	All pre-eclampsia	No pre-eclampsia
<30	120·2 (12)	— (–)	120·2 (12)	121·2 (5)
30–34	130·5 (11)	132·7 (6)	131·3 (17)	125·1 (11)
35–39	135·7 (29)	130·5 (11)	134·3 (40)	124·4 (43)
40–44	136·1 (24)	136·0 (17)	136·0 (41)	129·2 (44)
45–49	147·7 (14)	140·1 (22)	143·1 (36)	137·4 (38)
50–54	138·8 (8)	146·9 (13)	143·8 (21)	131·4 (21)
≥55	175·2 (5)	146·0 (4)	162·2 (9)	146·9 (15)
All	(103)	(73)	(176)	(177)

Numbers in parentheses = numbers in groups.

Blood-pressure of Siblings of Pre-eclamptics

The index cases in this part of the investigation are drawn from a series of women, aged 35 to 50 inclusive, who were interviewed during a follow-up study of blood-pressure up to twenty-one years after a first pregnancy (Adams and MacGillivray, in preparation). They fell into three groups: (1) those who had had severe pre-eclampsia in their first pregnancies, (2) those who had had mild pre-eclampsia, and (3) those who had been normotensive. At the interview, the names and addresses of all their brothers and sisters living in Aberdeen were requested. In the severe pre-eclamptic group the information was collected from as many index cases as could, or would, provide it, while in the mild pre-eclamptic and non-pre-eclamptic groups it was collected from as many as were needed to provide reasonable figures for comparison.

TABLE IV—MEAN DIASTOLIC PRESSURES (mm. Hg) OF SIBLINGS ACCORDING TO AGE AND BEHAVIOUR OF INDEX CASE IN FIRST PREGNANCY

Siblings' age (yr.)	Index case (first pregnancy)			
	Mild pre-eclampsia	Severe pre-eclampsia	All pre-eclampsia	No pre-eclampsia
<30	71·3	..	71·3	68·0
30–34	80·0	80·7	80·2	76·5
35–39	87·6	85·6	87·1	78·8
40–44	86·6	85·2	86·0	81·0
45–49	90·0	83·9	86·3	85·6
50–54	90·3	89·4	89·7	81·9
≥55	101·6	93·0	97·8	86·5

Numbers in groups as in table III.

A letter to the brothers and sisters in Aberdeen suggested various times when they could be visited in their own homes to have their blood-pressures measured. A total of 243 siblings was seen, and in each of the three groups the number comprised about half of all siblings alive. The age and blood-pressure of each sibling were obtained but not the weight or height. The technique recommended by the Cardiac Society and the American Heart Association (1939) for measuring blood-pressure was employed, after the subject had been sitting for ten minutes.

Tables III and IV give respectively the mean systolic and diastolic blood-pressures for all brothers and sisters

TABLE V—DISTRIBUTION OF BROTHERS AND SISTERS IN AGE-GROUPS

Siblings' age (yr.)	Index case (first pregnancy)					
	Mild pre-eclampsia		Severe pre-eclampsia		No pre-eclampsia	
	Brothers	Sisters	Brothers	Sisters	Brothers	Sisters
<30	3	9	..	..	2	3
30–34	3	8	2	4	3	8
35–39	7	22	3	8	9	34
40–44	6	18	2	15	10	34
45–49	1	13	6	16	5	33
50–54	2	6	3	10	10	11
⩾55	3	2	0	4	8	7

(including the index case) in age-groups, according to the behaviour of the index case in her first pregnancy.

Except for the mean systolic pressure in the under-30 age-group, the mean pressures of the siblings of pre-eclamptic index cases are higher than those of siblings of non-pre-eclamptic index cases. Taking into account the effect of age, the analysis of covariance shows that these differences are significant at the 0·1% level. There is little difference between the mild and severe pre-eclamptic groups. The higher pressures in the pre-eclamptic groups cannot be attributed to the inclusion of the index cases themselves, because the same pattern emerges if they are excluded.

The procedure of considering brothers and sisters together might be criticised on the ground that, below the age of 40 or so, men have higher blood-pressures than women, while above that age, the position is reversed; thus a preponderance of brothers under 40 and a preponderance of sisters over 40 could distort the means. The number of brothers and sisters in each age-group is, therefore, set out in table v (index cases being included as one of the sisters). It will be seen that in all but two age-groups the number of sisters exceeds that of brothers, of whom there are not enough to influence the means unduly.

Summary and Conclusions

Relatives of primigravidæ with and without pre-eclampsia and hypertension in pregnancy were studied.

One group consisted of the sisters of 146 primigravidæ who developed pre-eclampsia or some degree of hypertension in pregnancy. Examination of the records of these sisters showed that in their first pregnancy the frequency of pre-eclampsia or hypertension was higher than it was in the sisters of 273 primigravidæ without pre-eclampsia or hypertension.

Blood-pressure was estimated in 87 mothers who had at least one daughter with hypertension of some sort (severe pre-eclampsia, mild pre-eclampsia, or "other hypertension") in her first pregnancy, and in 149 mothers none of whose daughters had (so far as could be ascertained) developed hypertension in a first pregnancy. The mean systolic and diastolic pressures were significantly higher in the first group.

Siblings of 176 middle-aged women, who had had pre-eclampsia or hypertension in their first pregnancies some twenty years earlier, showed higher mean systolic and diastolic pressures, at all ages, than siblings of 177 middle-aged women who had been normotensive throughout their first pregnancies. No significant difference emerged between siblings of mild and severe pre-eclamptics.

These findings suggest a strong familial tendency to pre-eclampsia and hypertension in pregnancy. They seem to support the hypothesis that pre-eclampsia does not cause hypertension in later life but develops more often in women with an inherited hypertensive tendency, in whom hypertension would in any case have become manifest.

We wish to express our thanks to Sir Dugald Baird, Prof. Ian MacGillivray, Dr. Raymond Illsley, and other colleagues in the Obstetric Medicine Research Unit for their unfailing interest, encouragement, and constructive criticism; to Mr. W. Z. Billewicz and his staff for statistical analysis and advice; to Miss H. Genge-Andrews and Miss M. Milne for clerical assistance; and to the staff of Aberdeen Maternity Hospital for their invaluable cooperation.

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INFRA-RED THERMOMETRY IN THE DIAGNOSIS OF BREAST DISEASE

K. LLOYD WILLIAMS
M.Chir. Cantab., F.R.C.S.
SENIOR SURGICAL REGISTRAR

F. J. LLOYD WILLIAMS
M.B. Cantab.
RESEARCH ASSISTANT

R. S. HANDLEY
O.B.E., M.B. Cantab., F.R.C.S.
SURGEON

THE MIDDLESEX HOSPITAL, LONDON, W.1

IN a preliminary communication (Lloyd Williams et al. 1960) we reported observations on the measurement of infra-red radiation from the body-surface, and its modification by superficial pathological processes. We describe here in greater detail the application of infra-red thermometry to the diagnosis of swellings in the breast.

Theoretical Considerations

The skin is constantly emitting infra-red radiation. Since it behaves as an almost perfect black-body radiator, its infra-red radiation is in direct proportion to its surface temperature. Infra-red-measuring instruments can therefore be calibrated against a black body of known temperature so that they will give a direct temperature reading when exposed to the skin.

The advantages of measuring skin temperature from its infra-red radiation rather than by applying thermometric devices, such as thermocouples, to its surface are that infra-red measurement is almost instantaneous, that contact with the skin is unnecessary, and that an average reading over a small area is secured rather than the point reading which a thermocouple gives.

Skin radiates infra-red waves of between 4 and 20 μ with a peak of 9 μ. The Schwarz thermopile fitted with a calcium fluoride window, which was used for our observations, is sensitive within this range of infra-red waves.

In patients whose major blood-vessels are healthy, constant-temperature differences of more than 1°C between contralateral symmetrical areas of skin, in an individual who has been in a constant ambient temperature, indicate an abnormality in one or other area. To affect skin temperature, the abnormality must be either in the skin or just below its surface. Lesions in the breast are therefore particularly well suited to investigation by the changes which they cause in surface temperature.

Confusion has arisen between direct infra-red-wave measurement (which has hitherto scarcely been used in clinical practice) and the well-known method of infra-red photography. Infra-red photography depends on the waves reflected from the surface of the object photographed,

RESULTS OF INFRA-RED MEASUREMENT OF SKIN-TEMPERATURE IN 100 CASES OF LUMP IN ONE BREAST

Disorder	No. of cases	Temperature	
		Rise	No rise
Abscess	4	4	0
Carcinoma	57	54	*3*
Fibroadenoma	10	6	4
Cyst	18	*1*	17
Adenosis	6	0	6
Fat necrosis	2	0	2
Duct stasis	3	0	3

The exceptional cases of carcinoma and cyst are indicated by italic figures.

the waves being generated by an outside source which is much hotter than the body and therefore gives off very short infra-red waves. The measurements here described are of the longer waves which any object at body-temperature would generate; they are also much smaller in quantity than those given off by the sun or by lamps. The small volume of long-wave-length rays which the skin emits does not register on photographic emulsions.

Patients Investigated and Method

The series investigated consisted of 100 patients with a lump in one breast, admitted to the Middlesex Hospital between January, 1960, and January, 1961. There was an involuntary selection in favour of carcinoma since all clinically obvious cysts were aspirated in the outpatient department. The patients have all been followed for periods varying from six to eighteen months. With the exception of 2 advanced breast carcinomata, all diagnoses have been histologically confirmed.

The patient was taken into a small room, stripped to the waist, and laid on a couch with her arms behind her head (to prevent cross-radiation to the sides of the body from the arms). She was left for 15 minutes exposed to the ambient temperature. The breasts were then scanned with the thermopile, held in the observer's hand at 1 cm. distance from the surface. The measurements were taken in a constant pattern, radiating from the nipple. Each reading on one side was immediately followed by a reading over the mirror-image area of the opposite breast. Latterly the rectal temperature was also taken. The measurements were recorded on a proforma (fig. 1).

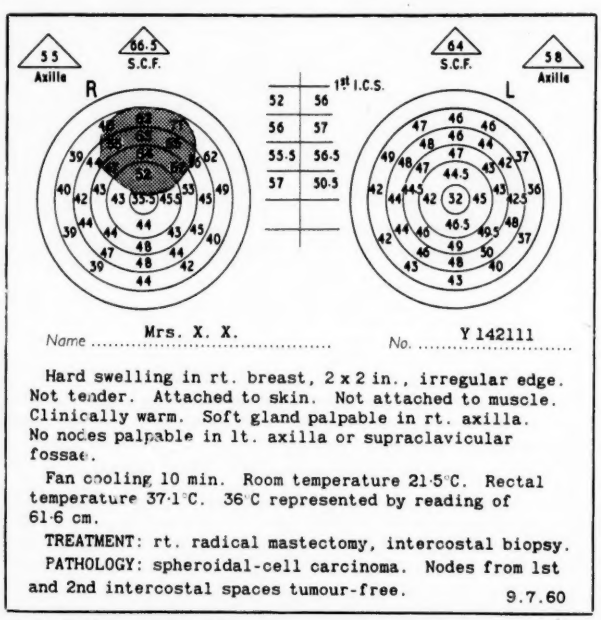
55
Axilla
66·5
S.C.F.
R
1st I.C.S.
52 56
56 57
55·5 56·5
57 50·5
64
S.C.F.
58
Axilla
L
Name Mrs. X. X.
No. Y 142111
Hard swelling in rt. breast, 2 x 2 in., irregular edge. Not tender. Attached to skin. Not attached to muscle. Clinically warm. Soft gland palpable in rt. axilla. No nodes palpable in lt. axilla or supraclavicular fossae.
Fan cooling 10 min. Room temperature 21·5°C. Rectal temperature 37·1°C. 36°C represented by reading of 61·6 cm.
TREATMENT: rt. radical mastectomy, intercostal biopsy.
PATHOLOGY: spheroidal-cell carcinoma. Nodes from 1st and 2nd intercostal spaces tumour-free.
9.7.60

Fig. 1—Proforma on which temperature readings were recorded, completed for a case of breast carcinoma.

The figures represent centimetres deflection on a mirror-galvanometer scale. Calibration against a standard heat source showed that the average temperature over the tumour was 2·5°C higher than the corresponding area in the normal breast.

S.C.F. = superclavicular fossa; I.C.S. = intercostal space.

Results

The results are shown in the accompanying table. Where a lump is shown as " hot ", this indicates an increase in the temperature of skin overlying the lump of more than 1°C compared with the same point of the opposite breast. " Cold " implies no difference in temperature over the lump compared with the opposite side, a rise of less than 1°C, or (as in 3 cases of cyst) an actual decrease of 1–2°C.

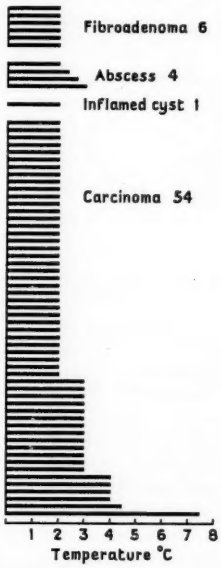

Fig. 2—Temperature range over 65 " hot " lumps.

57 carcinomata were scanned. 54 of them were hot, and among these were cases in which the clinical diagnosis was in much doubt. The 4 chronic abscesses were all hot. Of the 10 fibroadenomata examined, 6 were hot and 4 cold. Of the 18 cysts 1 was hot; the remaining 17 were cold, sometimes at a temperature lower than that of the normal side. The 6 cases of adenosis, 2 of fat necrosis, and 3 of duct stasis were all cold.

The range of temperature increase in the " hot " lumps is shown in fig. 2. The majority of carcinomata have a temperature increased by 1–2°C; that with a 7½° rise was a very large ulcerated tumour. The abscesses were all chronic, and none showed a rise greater than 4°C.

Discussion

The results of our observations, as set out in fig. 2, seem to divide breast lumps into two groups. The " hot " group consists of inflammatory swellings and malignant tumours. The " cold " group consists of degenerative processes. Midway between the two lie fibroadenomata, roughly half of which are " hot " and half " cold ". The exceptions in this classification consist of 3 carcinomatous lumps which were " cold " and 1 cyst which was " hot " —this finding was later explained by histological examination, which showed the cyst to be infected.

In practice this division into hot and cold groups may prove useful in diagnosis. Tumours and abscesses are usually easy to differentiate clinically, and infra-red measurements would give no help in distinguishing an inflammatory carcinoma from an infective process. A common problem is to decide whether a lump is a cyst or a small carcinoma, and here infra-red thermometry does give a useful indication. The 1 cyst, of the 6 investigated, which was hot indicates a difficulty. Subsequent histological examination showed the condition to be in reality a chronic abscess, because the cyst had become infected; but the infection was not clinically obvious. We do not wish to minimise evidence

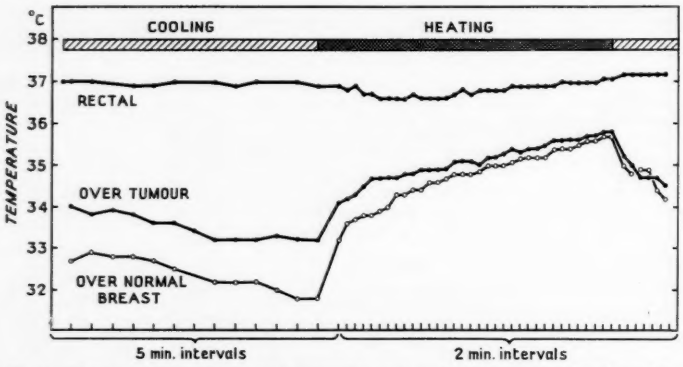

Fig. 3—Effect on skin temperature of warming and cooling a patient with spheroidal-cell carcinoma of breast (room-temperature 20·5°C).

contradictory to our view that infra-red thermometry is a useful investigation where the clinical diagnosis is in doubt.

The behaviour of fibroadenomata under infra-red measurement is ambivalent—half were hot and half cold. This is perhaps not of great clinical moment because the characteristic mobility of the fibroadenomata does not usually leave the clinician in diagnostic doubt. In general the more cellular fibroadenomata are " hot ", presumably because they are growing faster.

When the body is exposed to a constant ambient temperature somewhat lower than its own, its surface cools rapidly in the first 15 minutes. It continues to cool slowly for at least a further 45 minutes after which (in the absence of vasodilating influences or shivering) it achieves equilibrium with its surroundings. Areas of increased heat due to tumours do not cool relatively as much as normal skin; and we began to realise, as our observations proceeded, that the cooler the skin, the greater the temperature difference between a pathologically hot area and the contralateral normal area. Conversely, warming the patient diminished these differences. Fig. 3 shows this in graphic form from observations on a patient with a breast carcinoma. We now believe that it would be desirable to make all infra-red measurements in a room of constant and reasonably low temperature of perhaps 18°C, at which level the majority of patients do not shiver. In the absence of such a standard external environment, quantitative comparisons cannot be made; and, for example, it is impossible to relate the rise in temperature over a lump to the malignancy of the tumour—the difference is likely to be small when the external temperature is high. This may explain why 3 of the 57 carcinomata were " cold ": these 3 cases were investigated in the summer before the importance of a relatively cool body-surface was realised. Similarly the progress of a tumour—for example, its reaction to hormones or chemotherapy—could not be usefully investigated by infra-red measurements unless the recordings before and after treatment were made in an identical external temperature. It seems possible that if a standard-temperature room were available, infra-red thermometry might prove useful in following a tumour's response to systemic treatment. It is valueless for checking response to radiotherapy, at least during the early stages, because of the vascular changes which this treatment induces in all the surrounding tissues.

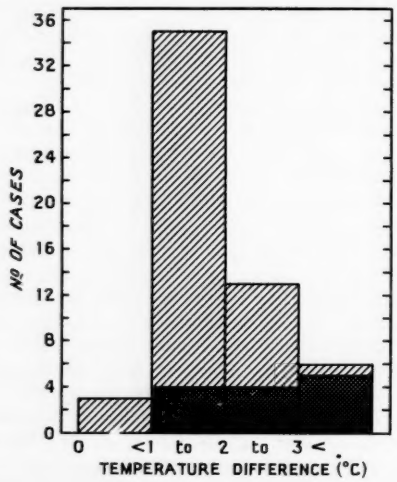

Fig. 4—Skin-temperature difference in 57 cases of carcinoma of the breast. Cases in which metastases subsequently developed are stippled.

Despite the importance of a standard ambient temperature in making comparisons between readings obtained on different occasions, there has been some correlation in this series between the degree of temperature rise and the malignancy of the tumour. Of the 57 carcinomata investigated 13 already show evidence of metastasis to the liver, bones, or lungs; and fig. 4 shows that such cases tend to be concentrated in the hotter groups. Thus of the 6 in which a temperature rise of 3°C or more was found, 5 now have secondary deposits. Of the 13 with a rise of 2–3°C, 4 show deposits; while of 35 with a rise of 1–2°C, only 4 have metastasised. The 3 cold carcinomata are free from recurrence.

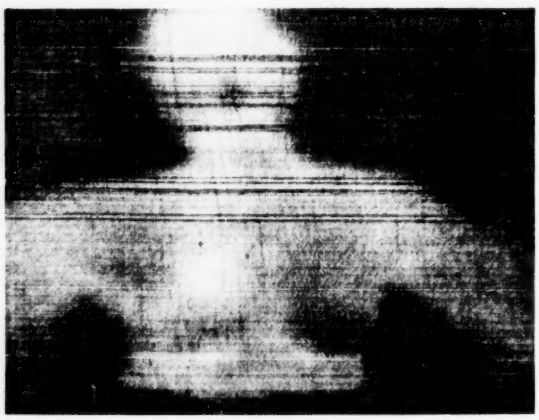

Fig. 5—Heat picture, produced by infra-red scanning, of a patient with carcinoma of the right breast. The light areas are " hot ".

It has long been known that an inflammatory carcinoma is obviously hot and that it has a very poor prognosis. It is also recognised that dilated subcutaneous veins may be present in a breast which harbours a carcinoma. It

appears, however, that Lawson (1957) was the first to observe that most carcinomata of the breast raise the temperature of the overlying skin. Much of the added heat is no doubt due to increased vascularity. But possibly metabolism in the tumour itself accounts partly for the temperature rise. One inflammatory carcinoma which we investigated was 5°C hotter than the opposite side and $^3/_4$°C hotter than the patient's rectal temperature. This is difficult to explain on a hypothesis of vasodilatation alone.

Since infra-red measurements can be made very rapidly and at a distance from the body, it is possible to construct an infra-red scanning device and record the results graphically. The infra-red detectors at present on the open market have neither the speed nor the sensitivity of response which light-sensitive detectors have. With more sensitive infra-red detectors it would be possible to show a picture of the body on a television screen in terms of heat, rather than light. By the courtesy of Messrs. Kelvin & Hughes, we have recently had a machine which has scanned the chest wall at 100 lines to the inch, and recorded the infra-red radiation on paper. It has in 4 minutes produced a picture of the body (which in its imperfection must resemble the first photographs ever taken) whereon was plainly demonstrated the hot area over a malignant lump (fig. 5). The scanning speed of this machine is slow. The sensitivity of its detector requires a difference of 2°C before the graphic record shows a difference recognisable by the eye. But, crude though it is, it does show that heat-scanning of an area of the body is practicable; and improvements in scanning speed and detector sensitivity could produce a new tool with great potentialities in clinical medicine and research, whereby the heat pattern of the skin could be made instantaneously visible.

Summary

The temperature of the skin can be determined almost instantaneously and at a distance by measurements of infra-red radiation from the body-surface.

100 patients with a lump in one breast were scanned with an infra-red measuring device. It was found that the skin overlying carcinomata and inflammatory lesions was hotter than the mirror-image area on the normal side; whereas the skin over degenerative lesions was cool. Such findings are sufficiently constant to be useful in diagnosis.

Some correlation was observed between the rise in temperature over carcinomata and their malignancy, judged by the clinical course.

Experiments with a mechanical scanning device, which has produced heat pictures of the thorax, suggest that it should become possible to visualise the body on a television screen in terms of heat, not light.

Our thanks are due to Mr. R. Elam and the directors of Messrs. Hilger & Watts Ltd., to Mr. C. M. Cade and the directors of Messrs. Kelvin & Hughes Ltd., and to Mr. B. Goff and the directors of Pye Tele-communications, who have instructed us and lent us apparatus.

We have had much help from Mr. M. S. Turney, F.R.P.S., and Miss E. Hewland, photographer and artist respectively to the Middlesex Hospital. We are grateful to our surgical colleagues for their interest and the clinical research committee for a grant to one of us.

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SODIUM-CHLORATE POISONING
COMPLICATED BY ACUTE RENAL FAILURE

R. C. JACKSON
M.R.C.S., F.R.C.P.E.
AIR COMMODORE

W. J. ELDER
M.B. Lond.
FLIGHT LIEUTENANT

H. MCDONNELL
M.B. Edin.
FLIGHT LIEUTENANT

From the Renal Unit, Princess Mary's R.A.F. Hospital, Halton, Bucks

POISONING by sodium chlorate ($NaClO_3$) in man is rare; so far as we know, only twelve such cases have been reported. Derot et al. (1948) described ten cases in patients seen between 1930 and 1946 (see accompanying table). Ehrhardt (1952) described two cases in which infants died after accidental poisoning with this chemical. Sodium chlorate has occasionally been used therapeutically in mouth-washes and gargles, but less frequently than the potassium salt. It is widely employed in industry and as a non-selective weed-killer. We understand that there has been no reported instance of human intoxication from its agricultural use (Edson 1961).

We record here the cases of two patients admitted to Halton in 1960 who recovered from sodium-chlorate poisoning associated with severe renal failure. Both required hæmodialysis (Kolff twin-coil artificial kidney). In one, poisoning occurred accidentally while the patient was using a weed-killer in an atomiser for agricultural purposes; and in the other it was the result of a suicide attempt.

Case-records

Case 1.—An experienced gardener, aged 48, on the morning of Jan. 25, 1960, used a very concentrated solution of sodium chlorate as a weed-killer in an atomiser. A strong wind was blowing the spray into his face. From time to time he was conscious of inhaling the spray and taking some into his mouth. That evening he felt unwell; he became nauseated and vomited.

The next day he complained of pain in the abdomen, vomited copiously, looked yellow, and passed no urine. On Jan. 27 he was admitted to the Essex County Hospital, Colchester, with a diagnosis of "acute abdomen". He was very ill, cyanotic, icteric, and anuric. The blood-pressure was 90/60 mm. Hg; blood-urea 148 mg. per 100 ml.; hæmoglobin 11·5 g. per 100 ml. (84% Haldane), and 57% of the hæmoglobin was present as methæmoglobin. The following day his condition had improved slightly after treatment with methylene-blue, neomycin, prednisolone, and penicillin; but the blood-urea had risen to 328 mg. per 100 ml., and he remained anuric. In addition he had pulled out an indwelling Foley's catheter with the bulb inflated, which caused urethral bleeding.

On Jan. 29 (5th day) he was transferred to Halton because of the renal failure. One striking feature was the slate-grey colour of his skin. The blood-pressure was 130/80. There were conjunctival icterus (serum-bilirubin 2·0 mg. per 100 ml.), epistaxis, moist sounds in both lungs, and an enlarged tender liver; and when the bulb of the Foley's catheter was released 180 ml. of pure blood came away. The hæmoglobin was 10·0 g. per 100 ml. (68% Haldane), red blood-cells 2,900,000 per c.mm. with 1150 normoblasts per c.mm. By Feb. 2 the reticulocytes were 3·4% and normoblasts 4000 per c.mm.

Fig. 1 shows the patient's progress and dialyses on the 6th, 11th, 18th, and 24th days of renal failure. He was completely anuric for 11 days. Because of the damage to the posterior urethra, suprapubic cystotomy was performed on Feb. 2, and this was closed on Feb. 26. It was not until the 31st day that a diuresis (>1 litre per 24 hours) began. The course of his illness was very stormy. The urethral damage did not prolong the oliguric phase. He lost 18 lb. (8·2 kg.) weight in 37 days, then regained 15 lb. (6·8 kg.) in 27 days before transfer back to

DATA ON 10 CASES OF SODIUM-CHLORATE POISONING (from Derot et al. 1948)

Case no.	Age (yr.)	Dose (g.)	Renal failure	Max. blood-urea (mg. per 100 ml.)	Result	Day of death	Reason for ingestion
1	?	30	–	..	Died	1	Dispensed in error for sodium-sulphate purge
2	?	30	–	..	,,	1	,, ,, ,, ,, ,, ,, ,,
3	30	?	?	..	,,	4	Sodium chlorate for weed-killer mixed in error with table salt
4	17	14	+	103	,,	2	Dispensed in error for sodium-sulphate purge
5	38	30	+	535	,,	10	,, ,, ,, ,, ,, ,, ,,
6	43	30	+	477	,,	10	,, ,, ,, ,, ,, ,, ,,
7	57	?	–	..	Survived	..	,, ,, ,, ,, ,, ,, ,,
8	?	?	–	..	,,	..	,, ,, ,, ,, ,, ,, ,,
9	?	?	–	..	,,	..	,, ,, ,, ,, ,, ,, ,,
10	17	20	+	640*	,,	..	Dispensed in error for sodium sulphate required for pustular spots

* Diuresis (1 litre) on 11th day.

his parent hospital on March 31, 1960, 67 days after the accidental poisoning.

Follow-up

The patient's progress has been reviewed by Dr. B. Moore-Smith, of the Essex County Hospital. In May, 1961, the patient had been back at full work for three months. The renal and liver function tests were normal, the hæmoglobin was 94%, erythrocyte-sedimentation rate 4 mm. in the 1st hour (Westergren), and blood-urea 28 mg. per 100 ml. A persistent urinary infection had cleared completely.

Case 2.—A housewife, aged 67, living alone felt perfectly well until about the beginning of August, 1960, when she became depressed. She first consulted her doctor early in October and was started on a course of imipramine (' Tofranil '). Over the ensuing fortnight she made good progress. During the evening of Oct. 14, on a sudden impulse she took a tablespoonful (about 14 g.) of a weed-killer, which contained 95% sodium chlorate, in about half a cup of water. She vomited almost immediately and continued to be sick over the next 48 hours. On Oct. 16 she passed about 30 ml. of very dark brown urine.

Her doctor was called on Oct. 17, and admission to Stoke Mandeville Hospital was arranged. Her general condition was quite good; a dusky tinge to the skin was noted. Her only complaint was of aching in both renal angles, which were tender. 30 ml. of dark red urine was obtained by catheter. The hæmoglobin was 46% and methæmoglobinæmia was found; the blood-urea was 245 mg. per 100 ml. Blood-transfusion (1000 ml.), methylene-blue, and ascorbic acid were given.

The following day the patient was transferred to Halton because of the acute renal failure. On admission she was conscious and well oriented. Her skin was a dusky slate colour, and there was an icteric tinge to the conjunctivæ. The blood-pressure was 160/70 with a regular pulse of 90. There was no œdema. The liver, spleen, and kidneys were not palpable. The hæmoglobin was 8·8 g. per 100 ml. (60% Haldane), methæmoglobin 0·9 g. per 100 ml.; red blood-cells 2,700,000 per c.mm., reticulocytes 0·8%; white blood-cells 22,300 per c.mm.; platelets 400,000 per c.mm.; serum-bilirubin 2·6 mg. per 100 ml.; liver-function tests within normal limits; blood-urea 260 mg. per 100 ml.; serum-potassium 5·0 mEq. per litre; serum-bicarbonate 24 mEq. per litre; serum sodium and chloride levels low. The urine volume was 50 ml. per 24 hours; specific gravity 1·011; urea 0·3 g. per 100 ml.

Fig. 2 shows the patient's progress. She was dialysed on the 8th and 17th days of renal failure. Diuresis (>1 litre in 24 hours) started on the 17th day. The hæmoglobin fell to 5·6 g. per 100 ml. (40% Haldane), but was raised to 11·2 g. per 100 ml. (80% Haldane) in stages by transfusions of blood totalling 2250 ml. By the 14th day from the poisoning the serum-bilirubin was normal. She did not lose the duskiness of her skin until just before discharge to convalescence on the 34th day after the suicide attempt. The total weight loss was $11^1/_2$ lb. (5·2 kg.).

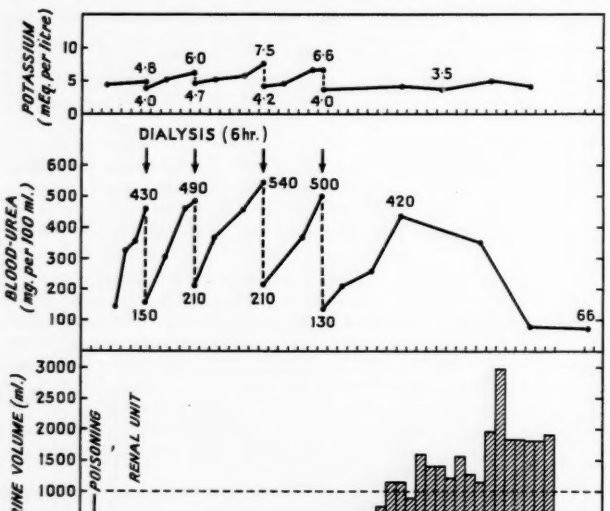

Fig. 1—Biochemical findings in case 1.

The effects of four dialyses are reflected in falls of serum-potassium and blood-urea. Suprapubic cystotomy was undertaken on the 9th day of renal failure. Diuresis began on the 31st day of renal failure.

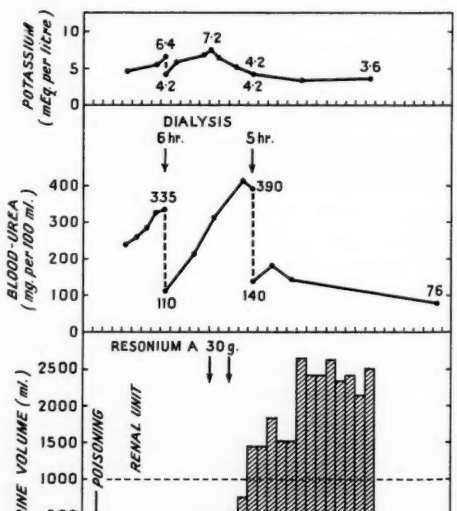

Fig. 2—Biochemical findings in case 2.

An ion-exchange resin (' Resonium A ') was given by mouth, 30 g. twice, to reduce the serum-potassium level. Diuresis started on the 17th day of renal failure when the second dialysis was performed.

Follow-up

When last seen at Halton in March, 1961, the patient was feeling and looking very fit. She had had no further depression. The weight had increased by 16½ lb. (7·5 kg.) since November, 1960. The urine was sterile, with a maximum specific gravity of 1·020 and a urea content of 0·86 g. per 100 ml.; the urea-clearance was 53% of average normal. Blood-urea 49 mg. per 100 ml. and hæmoglobin 11·2 g. per 100 ml. (80% Haldane).

Discussion

Sodium chlorate is of relatively low toxicity in both man and animals. It is a strong oxidising agent used commercially in the manufacture of explosives, matches, dyestuffs, tanning, and leather-finishing. It is also used commonly as a weed-killer. The lethal dose in man appears to vary considerably; for a 70 kg. man the oral dose is probably between 20 and 35 g. Early vomiting may reduce the effective dose, as in our case 2 who probably did not absorb all the 14 g.

Derot (1961) has seen one more case of intoxication with sodium chlorate since he reported those referred to in the table: it was administered to a man with criminal intent; he recovered. In the reported cases of poisoning with this chemical, it is surprising to find that all but one resulted from dispensing errors in which sodium chlorate was mistakenly dispensed instead of sodium sulphate or sodium chloride. Ehrhardt (1952) stated that the reason for the deaths of two infants was the use of abbreviations in ordering sodium chloride. The infants, aged 51 days and 45 days, were ordered Ringer's solution both intravenously and orally for the treatment of vomiting and pylospasm respectively. Both babies died within 48 hours. Sodium chlorate had been mistakenly used to make up the Ringer's solution. In the German practice:

Natrium chlorat = sodium chlorate
Natrium chloratum (or chlorat.) = sodium chloride

When ordering sodium chloride in bulk the dispensary had used the abbreviated form "natrium chlorat" but had missed out the full stop, and thus sodium chlorate was supplied.

The mode of accidental intoxication in our first patient is probably unique. From his detailed description of the incident, there is no doubt that, besides taking the spray into his mouth, he inhaled a considerable amount, causing irritation of his air-passages. He was having to blow his nose and cough frequently throughout the spraying operations.

In man sodium chlorate and potassium chlorate, taken internally, have three main actions, none of which is specific to these chemicals:

1. *General.*—As with many poisons local action brings about nausea, vomiting, and abdominal discomfort or colic, the severity depending on the amount taken.

2. *Hæmatological.*—An acute hæmolytic anæmia occurs, with varying degrees of bilirubinæmia. Hæmoglobin is reduced to methæmoglobin, which is transiently present in the blood. It is this pigment which gives the patient's skin the dusky slate-grey colour. This colour was most striking in our two patients, in whom it lasted some weeks.

3. *Nephrotoxic.*—Sodium chlorate, in common with mercury, sulphonamides, carbon tetrachloride, glycol, and other chemicals, is toxic to the renal tubules. The action of these chemicals in causing acute renal failure is twofold. First, a direct action on the tubules produces necrosis, and secondly an intense renal vasoconstriction causes tubular damage. The resulting lesion is a tubular necrosis.

Treatment

If the patient survives the first 48 hours of severe poisoning, renal failure it appears will be inevitable. Conservative treatment with a diet of carbohydrate and water which is free of protein, fat, and electrolyte should be started. If the renal failure continues, hæmodialysis will probably become essential. Davis (1956) described a case of renal failure following potassium chlorate poisoning which was successfully treated by conservative means; and case 10 in Professor Derot's series (see table) recovered despite a maximum blood-urea of 640 mg. per 100 ml. With modern management uræmia would not be allowed to advance so far; thus it is most likely that cases 5 and 6 in Derot's series would have been saved by an artificial kidney. We are certain our two patients, with oliguric periods of 31 and 17 days, would have died without hæmodialysis.

Summary

Two cases of sodium chlorate poisoning complicated by severe renal failure are reported. Both patients recovered with the aid of hæmodialysis. The first patient accidentally inhaled and ingested the spray from an atomiser while using a concentrated solution of sodium chlorate as a weed-killer. The second swallowed sodium chlorate in a suicide attempt.

All but one of the twelve reported cases of sodium-chlorate poisoning resulted from dispensing errors.

We wish to thank Dr. A. B. Pollard and Dr. V. E. Lloyd Hart for referring their patients to us; Dr. B. Moore-Smith for follow-up studies on case 1; Wing-commander F. H. D. Hutter for the surgical management of case 1; and the Director General Medical Services, Royal Air Force, for permission to publish.

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SERUM-CHOLESTEROL IN NORMAL SUBJECTS IN DENMARK

ERLING LUND
M.D. Copenhagen
ASSISTANT SENIOR PHYSICIAN

TORBEN GEILL
M.D. Copenhagen
MEDICAL DIRECTOR

GERIATRIC HOSPITAL, COPENHAGEN, DENMARK

P. H. ANDRESEN
M.D. Copenhagen
DIRECTOR OF THE BLOOD BANK, BISPEBJERG HOSPITAL, COPENHAGEN

WE have studied the serum-cholesterol levels of a large number of healthy youngish people in Copenhagen. In Denmark few studies of serum-cholesterol have been made, and they have involved a limited number of normal subjects:

In a series of predominantly youngish normal subjects, Teilum (1940) found the mean values were 169 mg. per 100 ml. for 165 males, and 178 mg. per 100 ml. for 129 females. Brun (1940), studying 9 males and 11 females aged 19–40, found that the values ranged from 145 to 280 mg. per 100 ml. Kornerup's normal series (1948) comprised 117 adults (80 males and 37 females); among the males the mean value in the age-range 19–46 was 203 mg. per 100 ml., and in the age-range 50–82 it was 237 mg. per 100 ml., and for females the corresponding values were 225 and 235 mg. per 100 ml.

Keys and Grande (1957) have reported the results of serum-cholesterol determinations on 95 males aged 40–49 in twenty-one population groups from various countries. The lowest mean values were found among Bantu Negroes (about 145 mg. per 100 ml.), and the highest in the United States (about 250 mg. per 100 ml.).

Subjects and Methods

Our investigations into serum-cholesterol levels were made on blood-donors from the blood-bank of Bispebjerg Hospital, Copenhagen:

The blood samples were collected between March, 1959, and March, 1960. All the samples were taken in the morning, but the subjects were not fasting.

The total cholesterol in the serum was determined by the Carr and Drekter method (1956), according to the principle of Liebermann Burchard, in the laboratory of the Geriatric Hospital.

The subjects totalled 2245 (1645 males and 600 females), and they ranged in age from 17 to 69. 1757 serum-cholesterol determinations were performed on the males, and 642 determinations on the females.

TABLE I—SERUM-CHOLESTEROL IN NORMAL MALES (1757 determinations) AND NORMAL FEMALES (642 determinations)

Age (yr.)	Serum-cholesterol (mg. per 100 ml.)								
	≦150	151–200	201–250	251–275	276–300	301–325	326–350	351–400	>400
Men:									
<20	6	28	3	..	..	..	..	..	..
20–29	33	231	147	23	3	7	1	..	1
30–39	5	154	334	80	47	11	2	3	1
40–49	3	91	192	94	39	17	5	3	1
50–59	2	16	84	34	21	4	4	2	..
60–69	..	3	7	4	5	..	..	..	..
No. of determinations	49	523	773	235	115	39	12	8	3
Women:									
<20	..	9	5	..	..	..	..	..	..
20–29	7	93	63	8	2	1	..	..	..
30–39	5	50	81	20	6	..	1	..	..
40–49	2	32	84	29	17	8	3	..	1
50–59	1	4	42	23	14	11	1	3	..
60–69	..	1	7	6	1	..	..	1	..
No. of determinations	15	189	282	86	40	20	5	4	1

Our Findings

The average values for our samples were 222 mg. per 100 ml. for the males, and 223 mg. for the females.

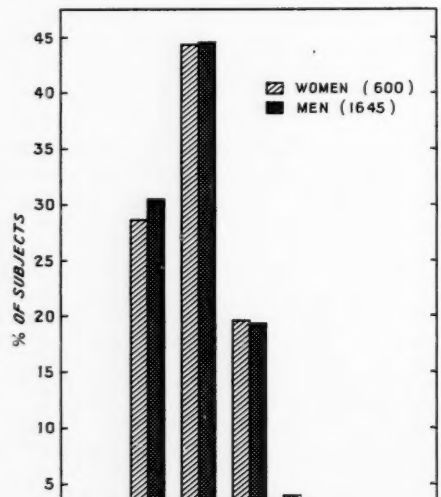

Fig. 1—Distribution of subjects according to cholesterol values.

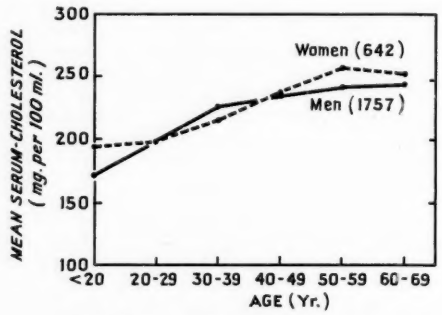

Fig. 2—Serum-cholesterol by age and sex.

Table I gives the serum-cholesterol values in 50 mg. groups from 150 to 400 mg. per 100 ml. Only a few subjects—male or female—had very low values (less than 150 mg. per 100 ml.) and very high values (more than 400 mg.). Most values were in the range 151–250 mg. per 100 ml. Fig. 1 shows the distribution of the subjects within the different groups of cholesterol values. The distribution for males and females is about the same, and the largest numbers fall within the range 201–250 mg. per 100 ml.

Fig. 2 sets out the mean values of serum-cholesterol by sex and age. Both curves rise with advancing age to a maximum of 244 mg. per 100 ml. for males, and 256 mg. for females, at the age of 60. In a previous study at the Municipal Geriatric Institution, Copenhagen, Geill, Lund, and Vedsø (1960) found that the mean values in a small elderly group (18 males and 24 females, aged 62–94) were 175 mg. per 100 ml. for the males and 232 mg. for the females. This decline of the serum-cholesterol level as age advances, especially among males, may be partly due to the mortality being higher among those with a high serum-cholesterol, but presumably the decreased food intake among elderly people is also operative.

TABLE II—HIGH SERUM-CHOLESTEROL VALUES IN NORMAL SUBJECTS

Serum-cholesterol (mg. per 100 ..)	600 women (%)	1645 men (%)
>250	24·5	22·6
>275	11·0	9·8
>300	5·0	3·4
>325	1·7	1·3
>350	0·9	0·6
>400	0·2	0·2

Table II shows the serum-cholesterol values which were higher than 250 mg. per 100 ml. in the present study. If 275 mg. is regarded as the upper limit of normal, about 10% of the subjects had high values, but few were above 350 mg. Sex differences were slight.

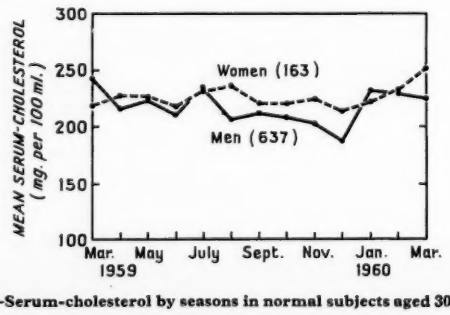

Fig. 3—Serum-cholesterol by seasons in normal subjects aged 30–39.

Fig. 3 shows the seasonal variations in mean cholesterol value for the age-group 30–39 years. These were exactly the same in the other age-groups, and there were no distinct fluctuations from month to month. Hansen (1954) and Norgaard (1956) have demonstrated a greater prevalence of coronary thrombosis during the winter months; on the basis of our findings, this cannot be a consequence of the serum-cholesterol levels being higher during the winter months.

Fig. 4 shows all the serum-cholesterol determinations distributed according to the days of the week. There was

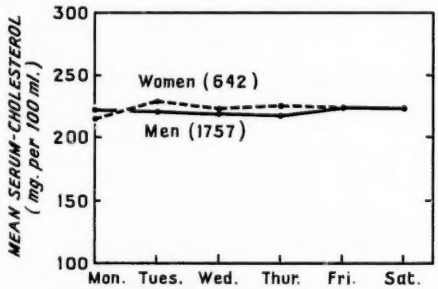

Fig. 4—Serum-cholesterol by days of the week.

no variation during the week, and in particular no increase on Mondays. Berg et al. (1957) and Jensen (1959) considered that myocardial infarction is more common after the weekend, and they suggested that it might be precipitated by high-fat meals on Sundays.

Summary

Determinations of serum-cholesterol in 2245 (1645 males and 600 females) normal subjects (blood-donors), aged 17–69, showed that the mean values were 222 mg. per 100 ml. in males and 223 mg. in females. Taking the upper limit of normal as 275 mg., about 10% of the subjects had high cholesterol values. 23% of the males, and 25% of the females, had levels which exceeded 250 mg. per 100 ml.

The mean value for serum-cholesterol increases with advancing age and seems to reach a peak around 250 mg. per 100 ml. at the age of 60.

Within the year that the determinations were made, the serum-cholesterol levels did not vary according to season or day of the week.

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"... psychiatry and psychology have been greatly blessed by *not* trying to measure things. They have often gone astray when they have tried to measure things because they find themselves measuring either the trivial or the irrelevant ... a science can be judged in one of two ways: by its predictable experiments and by its measurable phenomena, or by the closeness of fit between theory and observable fact. The latter, surely, is the kind of science that both Freud and Darwin were writing about."—Sir ROBERT PLATT, P.R.C.P. *Proc. R. Soc. Med.* 1961, **54**, 1083.

ACUTE BENIGN PERICARDITIS
DUE TO COXSACKIE VIRUS GROUP B, TYPE 3

DAVID LEWES
D.M. Oxon., M.R.C.P.
CONSULTANT PHYSICIAN

W. F. LANE
M.B., M.Sc. Dubl., D.P.H.
DIRECTOR, PUBLIC HEALTH LABORATORY

BEDFORD GENERAL HOSPITAL

IDIOPATHIC pericarditis was first described in 1854 (see Christian 1951); and in the past twenty years this condition, better known as acute benign pericarditis, has been more widely reported in the U.S.A., Scandinavia, and this country.

Since the isolation of Coxsackie viruses (Dalldorf and Sickles 1948), satisfactory proof, fulfilling the criteria laid down by Kilbourne (1952), that Coxsackie virus may cause acute benign pericarditis has been furnished in at least 13 cases (Kagan and Bernkopf 1957, Movitt et al. 1958, McLean et al. 1958, Roberts et al. 1959, Gordon et al. 1959, Null and Castle 1959, Sode and Baker 1959, Gillett 1959, Bell and Meis 1959, Brodie and Marchessault 1960). The majority of these reports have come from the U.S.A.; and none, as far as we can ascertain, have appeared in Europe. In the United Kingdom the only case-reports of probable Coxsackie virus pericarditis seem to be those of Fletcher and Brennan (1957, 1958) and Lane (1960).

We report here a case of acute benign pericarditis in which Coxsackie virus B3 was isolated and the level of neutralising antibody to this virus was significantly raised.

Case-report

In July, 1959, a 32-year-old bank clerk, while holidaying in Italy, began to have tight chest pain, a sense of pressure in the throat, and aching in the back and across the shoulders aggravated by change of position. He also complained of throbbing chest pain synchronous with the pulse. These symptoms persisted; and after some days pain aggravated by deep breathing prevented sleep. The patient obtained partial relief by standing still and at night by sleeping in a chair, the lying posture causing intolerable pain. Two weeks after the onset he was awoken by excruciating chest pain, which lasted 30 minutes. On admission to hospital on the same day (July 20, 1959) he said that he had had a similar but less severe illness in 1947, following acute appendicitis, while serving with the R.A.F. in Suez.

Examination

Temperature 100°F. Pulse-rate 90 per minute; pulse regular, of normal volume, and not paradoxical; blood-pressure 110/75 mm. Hg. There was a short thrill localised to the cardiac apex, which was in the normal position. A loud churning precordial pericardial friction rub, maximal over the mitral area and varying in intensity with respiration, obscured the heart sounds.

Treatment and Progress

The chest pain was controlled by analgesics; but two days after admission the patient complained of severe left-sided chest pain on breathing, and a loud pleuropericardial friction rub was heard. Fever, never exceeding 101°F, subsided after eight days, and within a fortnight he was symptom-free and all abnormal physical signs had disappeared. At follow-up two months after discharge from hospital he was well, and had had no recurrence of symptoms.

Investigations

Hb 90% (13·2 g. per 100 ml.); white blood-cells 6900 per c.mm.; erythrocyte-sedimentation rate (Wintrobe) 25 mm. in first hour (2 mm. on Aug. 24, 1959); antistreptolysin titre 56 units per ml.; blood-cultures (3) and throat swab negative; urine (spun specimen) normal.

Electrocardiography.—July 21, 1959: S-T elevation in standard and chest leads compatible with epicardial injury from pericarditis. July 30: symmetrical T-wave inversion V_2, V_3, and V_4; low or diphasic T waves in V_5 and V_6. Changes consistent with superficial myocardial damage. Sept. 22: appearance normal.

Chest X-ray.—July 20, 1959: very slight subsegmental consolidation and associated pleural reaction in the anterior basic segment of the right lower lobe. Left lung clear. Normal cardiovascular shadow. July 30: the area of subsegmental consolidation had completely resolved, but residual pleural thickening was present in the right costophrenic angle. Very slight subsegmental consolidation in the anterior basic segment of the left lower lobe. Cardiovascular shadow remained normal.

Virus studies.—A stool specimen was collected on the third day after admission to hospital—i.e., seventeen days after onset of illness. Culture in human amnion cells yielded a virus which was subsequently identified as Coxsackie virus Group B, type 3. Paired specimens of serum collected seventeen and thirty-three days after the onset of illness were examined for neutralising antibody by the tissue-culture method against the virus isolated from the fæces. The technique used was that of McLean and Cameron (1957) employing approximately 100 tissue-culture doses of virus and fivefold serial dilutions of serum. The titre of the serum was taken as the highest dilution which gave complete protection against the virus. The sera tested in parallel gave the following results:

1st specimen, day 14: titre 1:250
2nd specimen, day 33: titre 1:6250

Discussion

Acute benign pericarditis is probably commoner in Britain than the number of reported cases suggests (Swan 1960), and Coxsackie virus is likely to be a more usual cause than these reports indicate. It seems likely that the ætiology will be determined more often in the future as virological diagnostic methods come to be used as a routine.

There is already sufficient evidence to suggest that the terms "idiopathic" and "acute non-specific" pericarditis, which are inapplicable to Coxsackie pericarditis, might be replaced by "acute benign" pericarditis. According to Swan (1960) only 3 fatal cases with necropsy control have been reported. In 1, death was due to cardiac tamponade from serosangineous pericardial effusion; in the 2nd to cardiac tamponade from frank hæmopericardium following anticoagulant therapy; the 3rd patient died during induction of anæsthesia for unnecessary surgical intervention.

In adults Coxsackie pericarditis is rarely accompanied by severe myocarditis. The few reported cases (Fletcher and Brennan 1957, Weinstein 1957, Null and Castle 1959) have been due to Coxsackie B3, 4, or 5 infections; and all the patients recovered eventually. In infants and young children, however, Coxsackie myocarditis caused by infection with B3 or 4, or with B5 (Connolly 1961), is a serious and commonly fatal condition, especially in the neonatal period (*British Medical Journal* 1959).

Coxsackie-A virus, the ætiological agent in herpangina, has caused pericarditis in only 1 recorded case (Movitt et al. 1958). Coxsackie B, in addition to causing pericarditis and infantile myocarditis, is responsible for epidemic pleurodynia (Bornholm disease) and some cases of aseptic meningitis. Null and Castle (1959), reporting an outbreak of Coxsackie-B infection affecting 17 patients, found 11 with pleurodynia, 4 with pericarditis, and 2 with aseptic meningitis and meningoencephalitis. In the present case the illness was punctuated by pleuritic pain, and radiological evidence indicated evanescent pleural and parenchymal lung involvement. This patient's case was unusual in that it was apparently an isolated example of Coxsackie-B infection in the Bedford area; this strongly suggests that the patient was infected in Italy where the symptoms of illness began.

Summary

A case of acute benign pericarditis due to Coxsackie virus B3 infection in a man aged 32 is reported. Virus was isolated from the patient's fæces. Samples of serum taken during the acute and convalescent phases of the illness showed a significant rise in the level of neutralising antibody.

It is suggested that the terms "idiopathic" and "acute non-specific" pericarditis be replaced by "acute benign" pericarditis.

We wish to thank Dr. H. J. Hagger for reporting on the chest films, and Dr. J. H. L. Easton for advice.

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Preliminary Communication

ALTERATION IN SIZE OF NUCLEAR SEX-CHROMATIN MASS (BARR BODY) INDUCED BY ANTIBIOTICS

WHILE studying the Barr bodies of unfixed buccal smears, we noted that they were unusually small in one young woman. This observation was especially puzzling because on previous occasions her Barr bodies had had normal dimensions. Her menstrual history was normal and she was in good health. On further inquiry it was learned that recently she had been applying powdered chlortetracycline several times a day to her gums because of canker sores. On re-examination one week after discontinuance of the drug the Barr bodies were again normal. Studies were then instituted on the possible effects of pharmacological agents, particularly antibiotics, on the size of the nuclear sex-chromatin mass.

PROCEDURE

The subjects comprised four healthy women and one chromatin-positive man with Klinefelter's syndrome. Smears of the buccal epithelial cells were obtained by gently scraping the mucosa with a spatula. The scrapings were immediately mixed on a slide with 0·1% aqueous methylene-blue, and the mixture was allowed to stand for 5 minutes before a cover glass was applied and rimmed with 'Vaseline' to prevent drying. Such preparations were examined promptly under oil immersion at a magnification of ×1000. Rigid criteria were employed in the selection of Barr bodies for measurement. Only well-preserved cell nuclei containing sex-chromatin masses which were intensely stained, sharply defined, and in contact with the nuclear membrane were studied. The proportion of chromatin-positive nuclei in relation to the total

INFLUENCE OF VARIOUS ANTIBIOTICS ON DIMENSIONS AND PREVALENCE OF BUCCAL-SMEAR BARR BODIES

Antibiotic and number of days administered	Subject no.	Barr bodies				
		Before		After		
		Mean size (μ)	Prevalence (%)	Mean size (μ)	Prevalence (%)	Reduction (%) in mean area
Chlortetracycline:						
Topical, 2	1	0·78 × 1·2	20	0·52 × 0·76	20	57
,, 4	2	0·74 × 1·1	17	0·48 × 0·85	19	50
Oral, 4	1	0·75 × 1·1	19	0·58 × 0·84	18	42
,, 4	2	0·74 × 1·1	17	0·59 × 0·85	18	38
,, 4	3	0·65 × 1·18	20	0·54 × 0·85	19	40
,, 7				0·44 × 0·75	19	57
,, 4	4*	0·71 × 1·16	18	0·54 × 0·88	13	42
,, 7				0·52 × 0·96	17	39
Chloramphenicol:						
3½ ..	1	0·74 × 1·11	18	0·43 × 0·75	25	61
3½ ..	2	0·74 × 1·1	18	0·39 × 0·74	20	65
Benzylpenicillin potassium:						
3½ ..	1	0·83 × 1·2	23	0·46 × 0·77	18	64
4 ..	5	0·59 × 1·15	26	0·55 × 0·91	27	26
7 ..				0·50 × 0·77	28	43
11 ..				0·55 × 0·75	25	39
14 ..				0·47 × 0·70	28	51
Sulphafurazole:						
7 ..	2	0·73 × 1·1	19	0·58 × 1·05	16	29
12 ..				0·50 × 0·96	16	41

* Patient with chromatin-positive Klinefelter's syndrome.

number of technically satisfactory nuclei was recorded in each instance. Since the cells were unfixed and supravitally stained, their nuclear contents may be regarded as representing the living state at the time of measurement. For each Barr body a filar micrometer eyepiece was used to determine the major diameter and the greatest diameter at right-angles to this. At least 50 Barr bodies were measured in each preparation and the mean size of the sex-chromatin mass was calculated.

Such measurements, together with estimation of sex-chromatin prevalence, were made immediately prior to administration of the drug under trial, during and at the end of the treatment period, and again every 3–4 days thereafter until no further change in size was observed. To estimate the magnitude of the effect of the drug on the Barr bodies the approximate mean areas of the masses were compared before and after treatment and the percentage change was calculated.

Chlortetracycline (' Aureomycin ') powder was applied topically four times a day to the gums of one subject for 2 days and of another for 4 days. A 0·250 g. capsule of the drug was ingested four times daily for 4 days by two individuals and for 7 days by two others.

Chloramphenicol (' Chloromycetin ') was administered orally thrice daily in dosages of 1 g. for 3½ days to two subjects.

Benzylpenicillin potassium (' Pentids 400 '), 800,000 units, was ingested three times a day for 3½ days by one individual and for 14 days by another.

Sulphafurazole (' Gantrisin '), 1 g., was administered orally twice daily for 1 week, and increased to four times daily for the next 5 days, to one subject.

Each of two volunteers underwent four separate drug trials at adequately spaced intervals. Subject 2 received chlortetracycline, first topically and later orally, chloramphenicol, and sulphafurazole. Subject 1 differed in that she was given benzylpenicillin potassium instead of sulphafurazole. The patient with Klinefelter's syndrome (subject 4) received chlortetracycline. A total of eleven drug trials was carried out in the group of five individuals.

RESULTS

The results of these studies are summarised in the accompanying table. In no trial did any significant change in the prevalence of the sex-chromatin bodies occur. None of the subjects experienced untoward reactions to the antibiotics during or after any of the trials.

COMMENT

The normal range of variation in the size of the Barr bodies in the oral epithelial cells of a given individual is at present unknown. Moore and Barr [1] reported an average of 0·7 × 1·2 μ for 100 sex-chromatin masses (10 cells in ten smears) of normal females. The range of the observations in the same and different subjects was not recorded. The mean size of 0·73 × 1·15 μ for 700 supravitally stained nuclei in fourteen control preparations from the five individuals in the present study corresponds to that recorded by Moore and Barr surprisingly closely considering that a different type of preparation was used. Insufficient data have been accumulated as yet to indicate how widely the average diameters may vary between different persons or in the same subject at different times. Studies to elucidate this point are in progress. Preliminary observations suggest that the mean areas do not vary more than by 20–25% in the same subject over a long period, and do not differ by more than 30–35% between different individuals. It is reasonable to conclude that, in a given individual, changes of 30% or more in the mean area of the sex-chromatin bodies occurring within a week or two indicate an abnormal alteration of size. When reductions of this or greater magnitude occur repeatedly in direct relationship to administration of a pharmacological agent, we believe that this change can justifiably be ascribed to the drug. The validity of this notion is supported by data indicating a disappearance of the effect—that is, a return to normal size—shortly after discontinuance of the treatment.

The experimental data from eleven tests indicate that at least four chemically unrelated antibiotic compounds, administered in therapeutic dosages for periods ranging from 2 to 14 days, can induce a reduction in the size (see figure), but no change in the prevalence, of the Barr bodies in oral epithelial cells. This effect was usually noted within 4 days after treatment was started and disappeared soon after it was stopped. Prolonged treatment at a given dosage produced no further reduction. The effect of topical application of chlortetracycline to the gums was at least as great as that of oral administration. The sex chromatin of the chromatin-positive male with Klinefelter's syndrome reacted similarly to that of the healthy female volunteers.

The fact that the size of the Barr body can be influenced by extrinsic factors has a dual importance:

Jacobs et al.[2] observed " unusually " small and large buccal nuclear sex-chromatin bodies in certain phenotypic females with primary amenorrhœa, hypogonadism, and X-chromosome abnormalities probably associated with gonadal dysgenesis. While the abnormal size of the Barr bodies in these patients is most probably related to the loss or duplication of X-chromosome heterochromatin, it should be recognised that abnormal size of the sex-chromatin is not always due to genetic or intrinsic chromosomal aberrations.

Second, the reactivity of the Barr body to exogenous influences may provide an additional means of investigating its origin. Previous studies have concerned likely morphological precursors—i.e., the positively heteropyknotic heterochromatin of one or more X chromosomes. It now seems that additional information may be obtainable at the metabolic level, perhaps in terms of nucleoprotein biosynthesis.

This work is being extended to include larger numbers of subjects, other drugs, and the Barr bodies in epidermal

1. Moore, K. L., Barr, M. L. *Lancet*, 1955, ii, 57.
2. Jacobs, P. A., Harden, D. G., Buckton, K. E., Court Brown, W. M., King, M. J., McBride, J. A., MacGregor, T. N., Maclean, N., Fotheringham, A., Isdale, M. *ibid*, 1961, i, 1183.

as well as buccal cells. Preliminary observations indicate that the antibiotics employed in this study affect the sex-chromatin of skin cells in a manner similar to that noted in oral-smear cells.

We wish to express our deep appreciation to Mrs. Aimée Deitsch, Miss Jeanne Miller, Mrs. Marjorie Sherman, and Miss Jeanne Van Dyke for their splendid cooperation in serving as volunteer subjects for this study. We thank Lederle Laboratories for supplying 'Aureomycin', Parke, Davis & Company for supplying 'Chloromycetin', E. R. Squibb & Sons for supplying 'Pentids 400', and Roche Laboratories for supplying 'Gantrisin'. This work was supported in part by a grant from the Muscular Dystrophy Associations of America.

ARTHUR R. SOHVAL
M.A., M.D. Columbia
W. G. BRUCE CASSELMAN
M.A. British Columbia, M.D., PH.D. Toronto

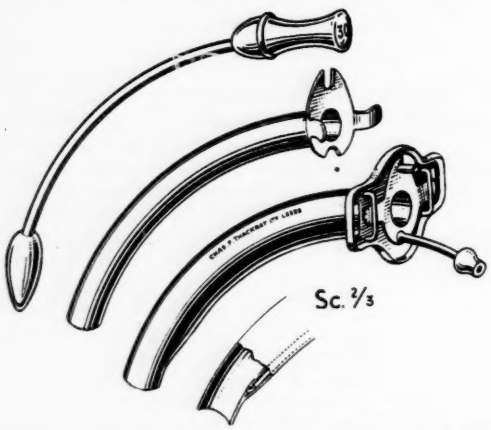
Endocrine Laboratory and Clinic, Department of Medicine, Mount Sinai Hospital, New York, N.Y., and Division of Cell Biology, Institute for Muscle Disease, Inc., New York, N.Y.

New Inventions

ADAPTATION OF THE CHEVALIER JACKSON TRACHEOTOMY TUBE

THE original Chevalier Jackson tube presents some difficulty because oxygen can only be administered through a catheter inserted into the lumen of the tube. Consequently, when suction is necessary, the administration of oxygen must cease. There is also the danger of the oxygen catheter being pulled out or falling out, and then being reinserted in a dirty and infected state.

The modification shown in the figure includes an additional tube fitted along the lesser curvature of the tracheotomy tube,



the two tubes being moulded together to give a smooth finish and to avoid grooves. The oxygen tube projects about 1/2 in. from the end of the tracheotomy tube, and the oxygen catheter can be fitted directly to it. At the inner end the oxygen tube joins the tracheotomy tube about 1/2 in. from its termination, so that the oxygen is mixed with air in the lower part of the tube and the stream of oxygen is not directed on to the wall of the trachea.

The advantages are that oxygen is administered separately, without danger of the oxygen tube being pulled out, and with continuous administration of oxygen during suction. The oxygen tube will take at least 6 litres per minute.

I am indebted to Messrs. Chas. F. Thackray, 10, Park Street, Leeds, 1, who have manufactured the apparatus, and from whom it can be obtained.

JOHN HOUSTON
M.B. Glasg., D.P.H.

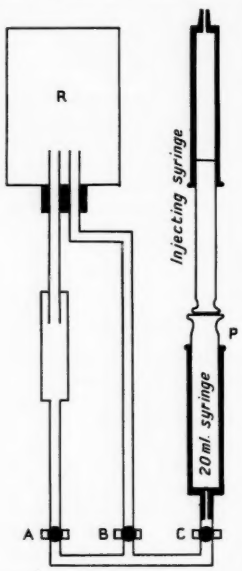
Aintree Hospital, Liverpool

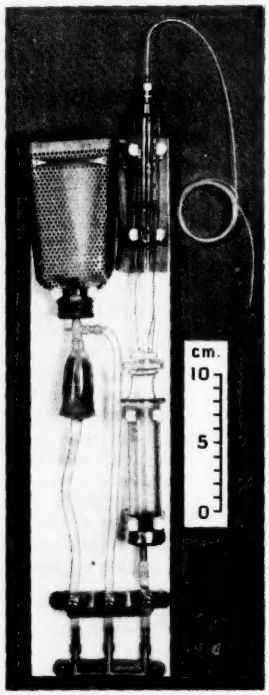
THE INFUSER

A Simple Hydraulic Press for Injecting Small Volumes of Fluid

THE apparatus consists of a flat screw-capped glass bottle (the reservoir, R) fitted with a rubber stopper (see diagram and photograph). A drip chamber without the filter of a disposable infusion set is connected to the bottle. Tubes from the drip chamber and air inlet are connected to the outer and central limbs of a metal E-piece, and a 20 ml. syringe is joined by a short length of polyethylene tubing to the third limb. Three tube clamps (A, B, and C) are placed proximal to the E-piece junctions. The whole apparatus is mounted on a rigid base, with Terry clips holding the reservoir and syringes.

With clamp A closed, and B and C open, water is repeatedly injected into the reservoir from the syringe, and clamp B is closed after each injection. Air trapped in the limb of the drip chamber is released and displaced by water from the reservoir on opening clamp A.





Complete filling of the reservoir is prevented by the pressure of the compressed air.

In use the apparatus is placed in an inclined plane with the reservoir uppermost. The plunger (P) of the 20 ml. syringe is completely depressed so that it is in apposition with the plunger of the injecting syringe. By opening C, and then gradually opening A, the compressed air in the reservoir drives water through the drip chamber to the syringe, displacing the plunger, which in its turn depresses the plunger of the other syringe. The drip-rate governs the speed of injection.

When the injection is completed, the water in the 20 ml. syringe is returned to the reservoir by opening B, closing A, and depressing the plunger. The apparatus is again ready for use.

Because the reservoir contains fluid under pressure, all junctions should be well sealed, and the reservoir protected. The plunger, P, should be well lubricated to ensure smoother action. Syringes of various size can be used with this apparatus, the injecting fluid being conveyed to the patient by polyethylene tubing.

Although this apparatus was designed for intravenous therapy, there is no reason why it should not be used for intra-arterial infusions.

A. E. NESLING
M.B. Brist.

Department of Anæsthetics, The Royal Infirmary, Bristol

Reviews of Books

An Introduction to Anæsthetics

JOHN D. LAYCOCK, M.B., F.F.A. R.C.S., assistant senior medical officer, South West Metropolitan Regional Hospital Board. London: Lloyd-Luke (Medical Books). 1961. Pp. 132. 10*s*.

UNDERGRADUATE instruction in anæsthetics is a controversial subject. There are the optimists who hold that any doctor on qualification should be capable of giving a simple safe anæsthetic; and there is the specialised postgraduate view of the pessimists. The practical needs of all countries in the world suggest that for many years most newly qualified doctors will at least occasionally need to know about the practical aspects of anæsthesia—and very certainly need to understand the closely related problems associated with the care of any unconscious patient. For these reasons, it is a pleasure to welcome Dr. Laycock's simple but sensible approach to anæsthetics. His book is designed for undergraduates and housemen, and contains the core of an essentially practical subject. Pharmacology and physiology in relation to anæsthetic apparatus and drugs, and unconscious patients, have necessarily been condensed. Such an approach, with its inevitable tendency to " talk down " to the reader, will irritate some, but on the whole it succeeds in its intention of underlining what is important for keeping the patient safely anæsthetised and at the same time satisfying the surgeon's needs. No book, however, can be a substitute for practical experience under supervision. Dr. Laycock's aim has been to provide a simple introduction to the first practical steps, and he has succeeded well. His publishers deserve particular praise for the excellent format at a very modest price.

Sir John Tomes: A Pioneer of British Dentistry

Sir ZACHARY COPE, M.D., M.S., F.R.C.S. London: Dawsons of Pall Mall. 1961. Pp. 108. 25*s*.

A CENTURY ago the dentists of this country were few, and they varied in their standard of education and ability. Some were ignorant charlatans, and others had received only empirical instruction while apprenticed to older men. There were, of course, physicians and surgeons who practised dentistry; but a training in this specialty was no part of their ordinary hospital experience, and any they received must have been fortuitous. The dentist of today owes the status and the corporate nature of his profession almost entirely to the efforts of one man, whose life is the subject of Sir Zachary Cope's book.

In 1836 John Tomes came from Warwickshire to King's College Hospital and the Middlesex Hospital, to read for a medical degree. Almost immediately he became preoccupied with a study of the structure and diseases of teeth, so that nearly twenty-five years elapsed before he sought a formal qualification. Long before then, however, his researches and his improvements in the design of many dental instruments had earned him an appointment to the staff of both the hospitals at which he had worked. And it was as a young but distinguished exponent of his craft that he began, in 1843, the fight to regularise dental surgery which was successfully concluded by the Dental Act of 1878.

This biography deals fully with Tomes' scientific discoveries and with the political labours which occupied the second half of his life. It also contains, reprinted in full, his classical article upon Fibrils of Soft Tissue in the Dentinal Tubes. Even Sir Zachary can supply little information about Tomes as a personality, but this attractive book will make many familiar with the story of a man who deserves to be remembered in more than the names of some histological minutiæ.

Robbery in London

An enquiry by the Cambridge Institute of Criminology, carried out by F. H. MCCLINTOCK and E. GIBSON. Edited by L. RADZINOWICZ. London: Macmillan. 1961. Pp. 147. 30*s*.

THE need for research into delinquency has long been apparent, but only in the past few years has it been undertaken on anything like an adequate scale. The Cambridge Institute, now firmly established under the direction of Prof. Leon Radzinowicz, has been responsible for several outstanding monographs, of which none is more interesting than a study of robbery in the London area—a model of careful investigation and scholarly presentation.

The recent major increase in robberies has not been in attacks on elderly or other defenceless people but in planned operations by small groups of professional criminals who have been all too successful in escaping unpunished. Robberies committed on private premises by experienced recidivists, together with planned coups such as bank raids and thefts of money or goods in transit, together constitute two-thirds of offences of this kind. Their steady increase indicates a sinister trend towards professional crime. Professional criminals undoubtedly have become more daring, and crime as a profession has become increasingly lucrative and safe. In 1950 nine out of eleven robberies involving property valued at over £500 were not cleared up by the police; and the figures for 1957 were no less than twenty-nine out of thirty. Moreover these figures refer only to known crimes.

The report shows clearly that robbers cannot fairly be categorised with the so-called violent criminals. In this country robberies in which the victims are assaulted or injured are still very rare; few robbers carry weapons and even fewer use them, and the use of firearms is virtually non-existent. The report points also to the inadequacy of our resources for dealing with criminals after conviction. Robbery, even without violence, can be punished with fourteen years' imprisonment but seldom is; and the main reason is awareness among the judiciary of the limitations of our penal institutions.

A Classification of Living Animals

Lord ROTHSCHILD, G.M., SC.D., F.R.S. London: Longmans, Green. 1961. Pp. 106. 25*s*.

THIS book was constructed as a working aid to help the author in his comparative studies of fertilisation. It enables him to put any animal in its right place in the natural order without doubt or delay. It begins with Phylum Protozoa, Class Mastigophora, Sub-class Phytomastigina (=Phytoflagellata), Order Phytomonadina (=Volvocina), Carteria &c., and ends with Phylum Chordata, Sub-phylum Vertebrata, Class Mammalia, Sub-class Theria, Infra-class Eutheria, Order Artiodactyla, Sub-order Ruminantia . . . It is the Order of Battle of the Animal Kingdom. Those who need this book will want it badly: it is beautifully printed, and it includes alternative classifications and an index which strikes the right balance between too much and too little. We have known an entomologist who qualified in medicine because the monetary rewards of chasing butterflies are meagre: a pathologist who devoted his time and his laboratory to the genetics of mollusca: a surgeon whose idle thoughts were of the comparative anatomy of the gallbladder—but not many of us are wise enough to make good use of this book.

Northern Rhodesia in the Days of the Charter

MICHAEL GELFAND, C.B.E., M.D., F.R.C.P., president, the British South Africa Company. Oxford: Blackwell. 1961. Pp. 291. 30*s*.

IN 1889 the British South Africa Company was granted a Royal charter, and the doubtful privilege of administering what was recognised to be one of the most unhealthy countries in the world. Even if Northern Rhodesia had not been full of malaria and sleeping-sickness, its climate alone was enough to justify the view that this was a land which white men could never, in the ordinary sense, colonise. There was no prospect of mineral wealth or other natural resources to offset the hostility of its inhabitants, and its only notable commercial traffic was in slaves.

Thirty-five years later, however, when the company transferred its responsibilities to the Colonial Office, an efficient medical service had been established, roads and railways had

been constructed through the bush, tribal warfare had been stamped out, and the slave trade had almost ceased. Copper, lead, and zinc had been discovered in large amounts, and the country was later able to make substantial contributions, in men and material, to the Empire forces during the first world war.

This book by Dr. Gelfand (a sequel to his earlier studies of Livingstone and of Southern Rhodesia) describes these formative years. In this story very many of the principal figures succumb to the endemic diseases of the young colony: and some who survive illness are killed by lions or attacked by natives. A valuable contribution to Commonwealth history, the book also does service in perpetuating the names of many doctors, members of either the company or the missionary societies, who richly earned commemoration.

On Detective Fiction and Other Things

G. F. McCLEARY, M.D., D.P.H. London: Hollis & Carter. 1960. Pp. 151. 16*s*.

FEW doctors reach the age of 94 and even fewer write books at that age. Dr. McCleary's life has been shared between music and preventive medicine with an occasional hour spent at Lord's. From being a church organist he passed via the College of Music (not yet Royal) to Trinity Hall, and in time became medical officer of health for Battersea. He writes of Cambridge in the '90s and of the origins of the infant-welfare centre. He paints an idolatrous picture of Sir Robert Morant and a kindly one of Bernard Shaw. He reminds us that the sixpenny doctor flourished until 1911 (and was not always a disgrace) and that there were fast bowlers before Lindwall (but none better). He displays Conan Doyle both as the only begetter of the incomparable Holmes and as a pertinacious fighter for human justice. But he reveals his heart most clearly when he writes of Bach's "forty-eight". This is not autobiography, but anecdotage at its best; and we wish there was more of it.

The Progress and Evolution of Man in Africa

L. S. B. LEAKEY. London: Oxford University Press. 1961. Pp. 53. 9*s*. 6*d*.

THE second of Dr. Leakey's lectures is a good short account of the place of the Olduvai discoveries in anthropogenesis; the first is a humane but rather discursive rebuttal of the brasher and more irrational allegations about African "racial inferiority". Dr. Leakey performs best when his advocacy is directed to pointing out that the patterns of tribal society are in many ways more socially effective than our own: elsewhere he gets involved in arguments which are mirror-images of the irrationalities he is attacking. The idea that Africa's counterpart to the intellectual achievements of the West "was the evolution of man himself" might have been sacrificed in favour of a few concrete facts about the remarkable and almost (to general readers) unknown city-States in African history—or to more about the tribal institutions and the culture of the Kikuyu, which Dr. Leakey knows so well. His point about the foolishness of expecting the peoples of Ghana or the Congo, who have traditionally chosen chiefs by acclamation, to behave like suburban electors is well made.

The Services of a Solicitor

The Law Society's Guide. H. J. B. COCKSHUTT. London: Hodder & Stoughton. 1961. Pp. 127. 2*s*. 6*d*.

LIKE general practitioners, solicitors must often wish that their clients would save themselves trouble by seeking advice earlier. This handy book sets out to explain to the public the services which solicitors can give. It projects the solicitor not only as the legal adviser on such matters as buying a house, entering into a partnership, or buying goods on hire purchase, but also as "an easily accessible and approachable person well fitted by his training, experience, and tradition to serve the public . . . in almost every field of human relations and endeavour".

The first of the book's two parts deals with the work of solicitors, the services they can render, their fees, and legal-aid schemes. The second part illustrates legal problems which may arise in everyday life on which the help of a solicitor may well be sought (why you should make a will, the motorist and the police, are you a member of any committee?, does your wife drive the car?).

Brain Mechanisms and Learning (Oxford: Blackwell Scientific Publications. 1961. Pp. 702. 70*s*.).—Reading the communications at this symposium, arranged by the Council for International Organisations of Medical Sciences in Montevideo in 1959, suggests at once that it was stimulating and interesting. The subject lies in a borderland between neurophysiology, psychiatry, and biochemistry, among other sciences—in a territory where many paths may prove to be fascinating dead-ends. Some of the work described was done on man, but most if it on animals—for instance, on the development of characteristic behaviour to find how far this was learned or innate, and to what extent bird-song patterns were inborn. It seems clear that many complex behaviour patterns are inborn; on the other hand, much is certainly due to conditioning, a type of learning which can on occasion be effective with astonishingly few conditioning stimuli. Some speakers discussed which parts of the nervous system are necessary for certain kinds of learning; and whether stimulation of any central or peripheral areas of the organism facilitates learning. The pathways and mechanism of conditioning received much attention: the electroencephalograph supplies some information about central activity during the process; and the state of endocrine activity of the organism has to be taken into account. Several speakers discussed learning from the viewpoint of synaptic efficiency and reverberatory circuits. It seems that structural changes result from the arrival of a stimulus; but so far it has not been possible to account for the durability in memory of a single event of, perhaps, little functional importance to the organism. Visual learning received attention, and, of course, the central reticular system. This is a valuable and well-produced record of an intricate and important subject.

Blacklock and Southwell: a Guide to Human Parasitology (7th ed. London: H. K. Lewis. 1961. Pp. 223. 30*s*.).—Brought up to date by Prof. T. H. Davey, who typifies the vigour and enthusiasm of the Liverpool School of Tropical Medicine, this book maintains its high level. It has always been notable for the accuracy of the text and the fine quality of its illustrations. This edition is no exception, and it has, moreover, been revised in almost every particular—e.g., by the inclusion of *Echinococcus multilocularis*, the alveolar hydatid, and by recent work on *Brugia malayi*, formerly *Filaria malayi*. In the brief section on larva migrans of the visceral variety, *Toxocara canis* is mentioned, but other forms of nematodes, especially the non-human filarioidea, have been omitted. This book can be heartily recommended as a guide for students and research-workers.

Whitaker's Almanack 1962 (London: J. Whitaker. 1961. Pp. 1189. 21*s*.).—This year the *Almanack* has again appeared with commendable promptitude, though 1961 has provided for the record a good many facts, both old and new. For one thing there was a Census, and all general and local figures from the preliminary reports have been included in this edition. Though only a youngish nonagenarian, the *Almanack* has seen our population double itself (23 million in 1871 and 46 million in 1961). But 1961 is more likely to be remembered for its spacemen than its Census, and there is a special article on artificial satellites, while detailed tables show their courses and indicate which are still in orbit. Coming nearer home, there is a new list of the more important museums and art galleries outside London. Another informative table, giving specimen rail fares for 1936-61, shows how much more it will cost to reach them.

THE LANCET

LONDON 23 DECEMBER 1961

Bouquet and Essence

EVEN some chemically-minded readers may be unaware that the first carbon atom of glucose is capable of combining with a number of phenolic bodies, the anthocyanidines (closely related to the tannins and flavanols), which differ among themselves only in the position of their hydroxy groups and their degree of methylation. From these unions arise a family of pigments (they include oenidine, cyanidine, delphinidine, petunidine, and peonidine) which, in proportions and permutations of inexhaustible variety, endow a Rosé d'Anjou with its gentle pink, a Clos de Vougeot with its full-blooded red, and a Lafite with that nobly fading purple which is no longer a colour but a vestment—*la robe*. These modest data represent one of the few successful excursions of modern science into the ancient and mysterious world of *la dégustation*. (It is a world in which the traveller from Anglo-Saxon lands is continually reminded of the limitations of his language: " wine-tasting " would be too crude a translation, " organoleptic assessment " perhaps a trifle *âpre*.) RANKINE et al.,[1] who examined 42 varieties of wine grapes (including the wild plant of Persia which is believed to be the ancestor of all European grapes), isolated nine different pigments; and in their detailed analysis of clarets the RIBEREAU-GUYONS [2] found mixtures of seven. But if the redness of red wine has had the sharp edge of its mystery (though not of its mystique) blunted, the whiteness of white wines—an even wider range from the delicate amber of a Chablis and the straw-like pallor of a Manzanilla sherry to the radiance of a Sauterne—remains a chemical enigma. " Given a single drop of petrol, the gas chromatographer can identify and determine a score of hydrocarbons," MARRISON [3] wrote recently in an entertaining and informative review, " and by mass spectrography it is possible to determine one part of boron in a thousand million parts of transistor silicone. But while the scientist cannot identify the vintage year of the wine, the expert taster can tell even the vineyard where the grape was grown. What is more, the taster enjoys the wine as he judges it."

Enjoyment must not, of course, be confused with mirth, or pleasure with frivolity. At last year's wine-tasting competition in Budapest, where 856 wines from 24 countries were assessed, the judges worked six hours a day for ten days " sans bruit, sans cesse, et sans défaillance ".[4] Only at intervals did they help themselves to fragments of dry bread or nibble a slice of apple to prevent their palates from becoming jaded; and every hour they rested for ten minutes, strolling in silent meditation around the cloisters of the ancient castle where the *dégustation* was held. Otherwise their time and attention was wholly devoted to the allocation of up to 20 points to each competitor: 2 for clarity, 2 for colour, 4 for bouquet, and the remaining 12 for taste, aroma, and general impression. The separation of bouquet from aroma should be noted: one concerns only the nose, the other refers to olfactory experiences from the moment the wine touches the palate. In a *vin ordinaire* the two may not be dissimilar; but with the great vintages the distinction becomes crystal clear. Crystal clear, that is to say, to the wine-tasting expert—not to the chemist or to the physiologist. Indeed, at no time is their incompetence more abjectly revealed than during those solemn moments when, the clarity of the wine having been assessed and its *robe* admired, the connoisseur lifts the glass to his nostrils. The predominant odour may be that of ethyl alcohol (though even this may be denied for the classical Rieslings of the Rhineland and it was surely inapplicable to Imperial Tokay); but the fragrance of a full Burgundy or the midsummer perfume of a Moselle are determined by a small residue of scented substances which scarcely amount to 0·5% of the wine's alcohol content. HENNIG and VILLFORTH [5] found in pentane extracts of Swiss wine (which retained most of the bouquet of the parent drink) principally butyl acetate and isoamyl acetate, with traces of vanilline, cinnamaldehyde, the esters of alcohols up to nonanol and of alpha-terpineol, and saturated aliphatic acids up to lauric ($C_{11}H_{23}COOH$)[5]; and others have attributed some influence to 2,3-butanediol, acetal, diacetyl, and the esters of carbonic acid.[6 7] But though simple variations in the number and proportion of these substances may explain tribal likenesses, they cannot account for the striking difference in bouquet between a burgundy, a hock, or a muscatel. The American Concord wine, a member of the indigenous *Vitis labrusca*, not of the *Vitis vinifera* family, contains methyl anthranylate, and its wine has a characteristic " foxy " odour and taste (which make it unsuitable for making champagne); but, since methyl anthranylate is also responsible for the far from foxy smell of oil of neroli, even this isolated chemical observation is of doubtful relevance. And yet the connoisseur will recognise with perfect assurance the " parfum très subtil de tabac fin " of a Conseillante 1950, the " parfum très fin de gibier " of a Le Caillou 1952, or the " grande originalité, notamment par son parfum discrèt de cuir russe " of a Lenclos 1952.

Even the comparatively simple phenomenon of turbidity and its effects on the general character of the wine are hard to explain in purely scientific terms. During cellaring, a complicated and skilled performance, all cloudiness due to yeast cells, pectinic and albuminoid bodies, excess of potassium hydrogen tartrate, and insoluble tannins should disappear; and a young wine

1. Rankine, B. C., Kepner, R. E., Webb, A. D. *Amer. J. Enol.* 1958, **9**, 105.
2. Ribereau-Guyon, J., Ribereau-Guyon, P., *C. R. Acad. Sci., Paris*, 1954, **238**, 2114.
3. Marrison, L. W. *May & Baker Lab. Bull.* 1961, **4**, 91.
4. *Revue du Vin de France*, 1960, **182**, 42.
5. Hennig, K., Villforth, F. *Vorratspfl. u. Lebensmittelforschung*, 1942, **5**, 213.
6. Peynaud, E., Lafon, M. *Ann. Falsif., Paris*, 1951, **44**, 263.
7. Kozenko, E. M. *Trud. Krasnodar. Inst. Pislichevoi Prom.* 1957, **9**, 47; *Chem. Abstr.* 1960, **54**, 21629.

lacking in clarity (usually as a result of inexpert ullage or of disease) is unworthy of the serious attention of either the chemist or the connoisseur. But the sickly muddiness of the stripling must not be mistaken for the venerable and revered crust of old age. This crust develops in the course of the slow second stage of fermentation, presumably as formerly soluble tannins are oxidised to insoluble compounds; but if the exact chemical nature of the original substances was imperfectly understood, their transformation only deepens the mystery. Chemists, on the other hand, might have little difficulty in explaining the " star-bright " limpidity of many modern standard wines: this is often their own rather than Nature's handiwork. It is possible to clear almost any wine by pasteurisation, refrigeration, multiple filtrations, and X-irradiation; but, alas, not all the skill of the laboratory can achieve this without some loss of flavour.

Flavour or the taste proper of a wine is perhaps the quality most difficult to analyse: and yet, if only the formidable language difficulties could be overcome, it might provide common ground to the chemist, the physiologist, and the gourmet. The number of constituents which endow a wine with its characteristic flavour is, in fact, small: the difficulty arises from the influence which each exercises over the perception of the others. Acidity usually becomes less obtrusive as alcohol content rises; a fall in pH impairs the appreciation of sweetness; and both sugar and glycerol tend to mask the astringency of tannin. Not only the final and indefinable balance but also the ineffable dignity of a Montrachet depends on the continuous interplay of the chemical reactions which each of its taste-giving constituents undergoes. The classical Emden-Meyerhof pathway, along which sugar is converted to alcohol, may be at the core of this transformation; but alcohol merely provides a canvas for the chemical artistry of the saccharomyces, the delicate grey down of fungi which flourish on the grape skin. What sugar they leave unfermented (it ranges from 4–5 g. per litre in a dry claret or burgundy to 60 g. or more in a d'Yquèm or an *Auslese* hock) is mostly glucose and fructose; but some rhamnose and arabinose (whose sweetness is slight) are also formed from the breakdown of pectins and glucosides. Hardly less important as a sweetener is glycerol, which is present in concentrations of up to 12 g. per litre in some wines. It is " the product of a side-slip of normal alcoholic fermentation " [3]: while most phosphodioxyacetone, the first open-chain compound formed, is converted to 3-phosphoglyceraldehyde, some of it is successively dehydrogenated and dephosphorylated. In addition to sweetness, glycerol enhances the smoothness, the body, and the keeping quality of wine—the reason for the abominable practice of *shellisage* (the addition of glycerol at a late stage of maturation), easily detected by both the chemist and the connoisseur. The cause of *amertume*, the slight and not wholly unpleasant bitterness of some modest burgundies, is probably the tendency of glycerol to decompose in a wine too low in alcohol content.

The biogenesis of a number of organic acids (mainly tartaric, citric, malic, and acetic) has never been completely elucidated; and even more mysterious is their unscathed passage through the turmoil of fermentation. Yet their effect on flavour is dramatic. Whatever a wine's other potential merits, too low a total acidity will make it *mou*, *plat*, or downright flaccid, while too high an acidity will make it unpleasantly sharp. (Chalk as a deacidifying agent has the sanction of ancient usage though not of French law.) Tartaric acid in particular imparts to a wine a characteristic pungency, though too much of it may make it *dur*. Malic acid, besides tasting like unripe apples when in excess, may be responsible for the tartness of a Chianti or the *panâche* of some of the best growth of the Médoc. Acetic acid, an essential trace constituent, is also the curse of many a fine vintage: a *vin tourné* can often be saved, despite its unpromising appearance, but the vinegariness of a *vin piqué* is usually beyond redemption. If most organic acids determine passing moods and social graces, tannin (related chemically to lignine) is the chief character-builder of wine. Its effect on flavour is difficult to define: and yet, like many builders of character, it is not without a touch of brutality. The unique, lingering glow, but also the initial *raide*-ness of a Châteauneuf du Pape may be its peculiar gift.

Together with some mineral matter (chiefly potassium phosphate), various nitrogenous trace substances, and residual glucosides, these compounds add up to the chemist's extract. As a general rule sweet wines have a high, dry wines a low extract; but there are exceptions in both classes. To a connoisseur, moreover, a dry wine with a high extract can be anything from *charnu* or *etoffé* to plain *corsé*; and only the most intrepid chemist would maintain that *corsé* is the exact equivalent of *charnu* with an extra dash (say 1%) of ethyl alcohol added to it.

The Blood-brain Barrier

THE concept of a blood-brain barrier—to be distinguished from a blood/cerebrospinal-fluid barrier —emerged from the work of EHRLICH in 1885 on the staining of tissues by circulating vital dyes. The failure of such dyes to colour the central nervous system contrasted sharply with the readiness with which they stained other organs. According to an interpretation long placed on these observations, the endothelium lining the fine blood-vessels of the brain differed significantly in permeability from the cells of capillary walls elsewhere. It was only at sites of injury, such as those associated with inflammation or anoxia, that damaged cerebral vessels allowed the leakage of the dye from the blood-stream. More recently, with fuller knowledge of the intimate structure of the central nervous system,[1] it has been suggested that the supposed mechanical barrier may be formed not only by the endothelium but also by many surrounding neuroglial cells whose attached feet cover much of the exterior of the cerebral capillaries. There are, however, other possible explanations for the exceptional behaviour of

1. Peters, A. *in* Cytology of Nervous Tissue. Proceedings of the Anatomical Society of Great Britain and Ireland; 1961, p. 20.

the central nervous system in this respect, of which two will be briefly described.

First, it is possible that, as long as the cerebral tissues are normal, substances fail to escape from the small blood-vessels, not owing to any endothelial cell barrier, but because in the brain there is little or no extravascular space to receive them comparable with that in other organs.[2] One of the main difficulties in regarding the brain as peculiar in this respect is the high concentration in the cerebral tissues of sodium and chlorine [3]—both elements that in other organs are believed to be almost wholly extracellular. Moreover, the concentration of chlorine in the brain tissues can be raised or lowered by introducing into the blood hypertonic sodium chloride or osmolar glucose solutions respectively; in such experiments, the changes in the composition of the brain substance follow closely those in the plasma.[4] Consequently, if it is assumed that the central nervous system has hardly any tissue-fluid space, then presumably its cells must normally contain much sodium and chlorine in a freely diffusible form —which seems unlikely. Until we are more certain as to the presence or absence of tissue-fluid ir the central nervous system—some physiologists believe that it is present, whereas some electron microscopists deny this [5–7]—the explanation of the observations by EHRLICH and others that has been based on a peculiar impermeability of the cerebral capillaries must remain in question.

Second, should the belief be correct that a tissue-fluid exists in the central nervous system comparable with that found elsewhere, molecular size, coupled perhaps with molecular electrical charge, may not be the sole factor determining the escape or retention of a substance in the blood as it passes through the brain. That molecular size is operative here as in other organs seems likely from the finding that normally, proteins, whether homologous or foreign, do not pass through the walls of the brain capillaries. This is readily substantiated by studies with proteins labelled with radioactive isotopes.[8] On the other hand, small molecular size alone does not seem to account for the escape of substances from the blood. Whereas some substances of small molecular size, such as glucose, alcohol, volatile anæsthetics, and barbiturates, seem to gain access readily to the brain substance, others of equally small size—for example, the hexose carbohydrate fructose —do so much more slowly.[9]

This difference in the behaviour of small molecules has made it necessary to amplify [10] the concept of the supposed barrier as consisting of a simple mechanical sieve. It has been suggested that some substances of small molecular weight naturally present in the blood (among them some aminoacids and inorganic phosphate) enter the brain less readily than other tissues because normally they participate to a lesser extent in the brain's metabolism, whereas others (notably glucose), on which neuronal activity almost wholly depends, escape freely from the blood.[11] To summarise this view, which has been strongly supported by DOBBING,[10] large molecules are probably retained in the blood because of their size and perhaps because of their electrical charge, while small molecules escape or fail to escape according to whether or not they participate significantly in cerebral metabolic processes.

Although EHRLICH's observations have been repeatedly confirmed, there are now known to be several sites in close relation to the central nervous system at which the barrier against vital dyes seems to be less effective. Of these two, the *areæ postremæ* in the floor of the fourth ventricle [12] and the posterior spinal ganglia [13] seem to be especially important. PAYLING WRIGHT [14] has suggested that at these apparently more permeable sites, toxic substances and neurotropic viruses may make their initial entry into the central nervous system.

2. Maynard, E. A., Schultz, R. L., Pease, D. C. *Amer. J. Anat.* 1957, **100**, 409.
3. Ames, A., Nesbett, F. B. *J. Neurochem.* 1958, **3**, 116.
4. Woodbury, D. M. *in* Biology of Neuroglia (edited by W. F. Windle); p. 120. Springfield, Ill. 1958.
5. Farquhar, M. G., Hartmann, J. F. *J. Neuropath. exp. Neurol.* 1957, **16**, 18.
6. Schultz, R. L., Maynard, E. A., Pease, D. C. *Amer. J. Anat.* 1957, **100**, 369.
7. Horstmann, E., Meves, H. *Z. Zellforsch.* 1959, **49**, 569.
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Annotations

INFRA-RED THERMOMETRY

ALL objects with a temperature above absolute zero radiate infra-red rays. The warmer they are the shorter the waves generated and the greater their quantity. Very hot objects emit waves so short that they begin to enter the visible spectrum and are recognised by the eye as red hot. Those of us who still retain a few rags of our 1st M.B. physics will recollect that surfaces vary in the efficiency with which they radiate heat, the most effective being a dull black surface. The radiation of infra-red waves (i.e., *radiated* heat) from an efficient radiating surface is in direct proportion to its temperature, and infra-red detectors can therefore be used as thermometers for estimating the temperature of such surfaces at a distance. The skin radiates heat with an efficiency close to that of a black-body radiator, and its surface temperature can therefore be measured, by suitable infra-red detectors, with great speed and without the need for contact between the skin and the instrument.

Because of the importance of infra-red detection in Defence, much of what is known about it is secret, and the top soil of its application in medicine has hardly yet been scratched. Lawson [15] seems to have been the first to point out that infra-red measurements can be used to detect superficial carcinomata, and in this issue Mr. Lloyd Williams and his colleagues report on their infra-red investigation of lumps in the breast. They found that abscesses, some fibroadenomas, and nearly all carcinomas in the breast give rise to a localised " hot spot " over the lumps they form, whereas cysts do not show a significant rise of skin temperature.

Though most of their work so far has been done with a

detector manipulated by hand, they are now working with a heat-scanning device which will produce a crude picture of the body surface in terms of its radiated heat and not its reflected light. If this device can be rendered more sensitive and more rapid, it might have many applications besides that of distinguishing a breast cyst from a breast carcinoma.

BROILERS

IT was about ten years ago that we heard the first whispers of a plot to pervert the Englishman's traditional tastes in table poultry. He had to be made " broiler conscious ". The conspiracy has succeeded, and today the production of birds for the table is organised on industrial lines. Batches amounting to several thousand birds of selected breed and of the same age are reared on a physiological diet in premises with controlled temperature and humidity. On a day decided by biometrical statistics (and perhaps a glance at the market prices) they are killed, bled, plucked, eviscerated, cooled, trussed, packed, and put in the cold store. The costs are lower; the meat is more tender; and the housewife, when she buys the final product, believes that she is in the swim. The whole business is a triumph for the geneticist and the organisation man.

Two main troubles have plagued the pioneers. A community of coetanean birds, like children at a boarding-school or a barrack-room of raw recruits, is singularly liable to murrains. Some of these can be prevented by care in breeding, and some (e.g., coccidiosis) can be controlled by chemotherapy. Some diseases, however, can convert modest profit to total loss within forty-eight hours. Given the best of care a few infected birds may reach the end of their short lives without overt signs of the disease but still harbouring the causal bacteria in their bowels.

The traditional methods of the farmhouse kitchen are unsuitable for preparing several thousand birds for market. It is now generally agreed that from killing to packing the work must proceed on a linear plan, and a Code of Practice [1] issued by the Ministries of Health and of Agriculture, Fisheries, and Food is designed to help large and small producers in the design of new premises and the operation of existing ones. It is full of sound advice. The guiding principle is to keep " clean " and " dirty " apart just as they are separated in the operating-theatre. The Rubicon is reached when the bird has been plucked—mechanically, of course—and before its guts are removed. The period of greatest danger is immediately after evisceration, and perhaps the Code might have been more explicit about this danger. Dixon and Pooley [2] have shown that the edible meat may be contaminated by the contents of the bowel at two points: during evisceration, and when the carcasses are chilled in a water-bath immediately afterwards. Contamination at the first of these points may be avoided by strict compliance with the recommended drill: at the second, if one or two carcasses are infected a whole batch may be contaminated. Water containing 5 p.p.m. of free chlorine is recommended for the cooling bath but experiments have shown that even 200 p.p.m., though helpful, is not enough to sterilise the ice-water mixture.[3] There are less crude ways of chilling the birds, and it is to be hoped that they are adopted generally. The Code pays attention to the collection and disposal of the byproducts of the industry—blood, feathers, and inedible offals. These are too valuable to be destroyed, but unless removed from the premises expeditiously are liable to be an insanitary nuisance.

This is the fifth of these Codes of Practice to be published, and we must assume that though they have no legal teeth they are an influence for good. The buyer of a " broiler " or *poussin* runs fewer risks than he did in the early days of the trade. As in the milk trade the small producer is being submerged by a few large companies which have both the brains and the capital to set a high standard and which know that there is no profit in sour milk, contaminated poultry, or decayed offals.

1. Poultry Dressing and Packing: Food Hygiene Codes of Practice no. 5. H.M. Stationery Office, 1961. Pp. 35. 1s. 3d.
2. Dixon, J. M. S., Pooley, F. E. *Mon. Bull. Minist. Hlth Lab. Serv.* 1961, **20**, 30.
3. Dixon, J. M. S., Pooley, F. E. *J. Hyg., Camb.* 1961, **59**, 343.

AWARENESS DURING SURGERY

NOT uncommonly patients ask the anæsthetist for an assurance that they will not wake up during a surgical operation. Since the introduction of the relaxant drugs very light general anæsthesia has become increasingly popular; nitrous oxide, with a relatively high concentration of oxygen, is usually the main agent employed for maintaining unconsciousness. Most anæsthetists believe that a mixture of 70% nitrous oxide with oxygen, particularly when combined with hyperventilation by controlled respiration,[1] suffices to maintain analgesia and anæsthesia during surgical procedures. Premedication with morphine or pethidine combined particularly with scopolamine,[2 3] induction with thiopentone, and the use of adjuvant anæsthetic or analgesic drugs diminish the likelihood of the patient's appreciating his surroundings during operation.

In 1950 Winterbottom [4] described the case of a patient who recalled waking up and feeling pain during an operation. Since then, isolated reports of similar occurrences have appeared. Hutchinson [5] has now described her investigation of over 650 patients. 8 apparently awoke during operation, and 4 of these felt pain or discomfort; but in only 2 cases was it possible to corroborate that the experiences recounted were in fact memories of actual happenings and not just dreams. All 8 were women (although this may not be significant). There were many different anæsthetists and techniques, but features common to all 8 cases were administration of nitrous oxide and oxygen (with or without halothane or small intravenous doses of pethidine) and of muscle relaxants and intermittent positive-pressure respiration. An inadequate concentration of nitrous oxide (where known it was 70–75% in oxygen), resistance to nitrous-oxide anæsthesia, and failure to maintain hyperventilation are all discussed as possible reasons for return of consciousness. It seems likely that, if very light anæsthesia is used, complete control of respiration is preferable to " assisted " respiration. Passive hyperventilation produces a state of indifference to environment and raises the pain threshold [6]; cerebral vasoconstriction occurs, but no permanent brain damage has been demonstrated. It has even been suggested that a low CO_2 tension may affect the reticular system in the brain. Other factors which may play a part are the antianalgesic action of thiopentone [7 8] and certain pheno-

1. Geddes, I. C., Gray, T. D. *Lancet*, 1959, ii, 4.
2. Dundee, J. W., Moore, J. *Anæsthesia*, 1961, **16**, 194.
3. Lambrechts, W., Parkhouse, J. *Brit. J. Anæsth.* 1961, **33**, 397.
4. Winterbottom, E. H. *Brit. med. J.* 1950, i, 247.
5. Hutchinson, R. *Brit. J. Anæsth.* 1961, **33**, 463.
6. Robinson, J. S., Gray, T. C. *ibid.* pp. 62, 69.
7. Clutton-Brock, J. *Anæsthesia*, 1960, **15**, 71.
8. Clutton-Brock, J. *ibid.* 1961, **16**, 80.

thiazine derivatives such as promethazine,[9 10] the poor analgesic properties of low concentrations of halothane,[11] dilution of the gas mixture owing to leaks in the apparatus,[12] and the difficulty of recognising a lightening of anæsthesia in the paralysed patient. Movements, sweating, and bronchospasm may indicate that the level of anæsthesia is too light; but patients may afterwards complain even though no sign of returning consciousness has been observed.[5] Amnesia may imply that a painful episode has been repressed [13]; on the other hand, some patients recall incidents without any apparent distress. Recent work has shown that the effect on consciousness of a given tension of nitrous oxide or carbon dioxide in the blood varies from one individual to another.[14 15] Careful electroencephalographic studies are needed.[16]

Nitrous oxide with adequate oxygen is a very safe but very weak anæsthetic agent; and Hutchinson's paper is a timely reminder that muscular relaxation and hyperventilation, insufficiently supplemented by more powerful drugs, will, not invariably ensure unconsciousness. The anæsthetist's first concern is to relieve pain and apprehension, and the patient should be assured of oblivion as well as of safety.

PAROXYSMAL HYPERTENSION OF CEREBRAL ORIGIN

PHÆOCHROMOCYTOMA is the most widely recognised cause of paroxysmal hypertension; and it is probably the most important cause to recognise, because timely removal of the tumour effects cure. Of the many other causes of paroxysmal hypertension most are neurological disorders. A common benign type is that found in young or middle-aged people with an unusually labile autonomic nervous system. Unfamiliar experiences, such as a medical examination, and particularly the recording of the blood-pressure, evoke signs of overactivity of the sympathetic nervous system such as palpitation, sweating, and erythema over the upper chest and root of the neck; and this is accompanied by an abrupt rise in blood-pressure. These patients, with what Page[17] has designated " the hypertensive diencephalic syndrome ", have no organic disease and need no treatment. Indeed, hypertension associated with tachycardia and " necklace erythema " can usually be disregarded.

Paroxysmal hypertension may also betoken organic brain disease. It has been described in tabes dorsalis,[18] usually in association with other evidence of sympathetic-nervous-system discharge. There is a classical account by Penfield[19] of paroxysmal hypertension associated with a ball-valve tumour intermittently blocking egress of cerebrospinal fluid from the lateral ventricles. Montgomery[20] has lately drawn attention to paroxysmal hypertension in basilar-arterial insufficiency. He describes three patients over the age of 60 with blood-pressures at or a little above the upper limit of normal in whom the systolic pressure rose abruptly by 90 mm. Hg and the diastolic pressure by 60 mm. Hg; the hypertension lasted for periods varying from seven minutes to four hours and then fell to the previous level. These attacks were repeated over a period of weeks or months, later becoming associated with signs of periodic medullary dysfunction such as dysphagia, dysarthria, and weakness and sensory disorders of the limbs. All three patients ultimately developed signs of medullary infarction; and in one this was seen at necropsy, which also showed a grossly sclerotic basilar artery and abnormally small posterior communicating branches of the circle of Willis. The absence of a chromaffin tumour was confirmed in this patient. In these three cases, when the paroxysms of hypertension were associated with neurological crises, the diagnosis of basilar arterial insufficiency was not difficult; but in two the earlier paroxysms were not accompanied by any neurological signs. The fact that all three patients died under observation suggests that this syndrome carries a grave prognosis. Millikan and Sickert[21] gave a good description of the syndrome of intermittent insufficiency of the basilar arterial system. They treated six of their ten patients with anticoagulants, and their experience suggested that this treatment was beneficial.

9. Moore, J. Dundee, J. W. *Brit. J. Anæsth.* 1961, **33**, 3.
10. Moore, J., Dundee, J. W. *ibid.* p. 422. See *Lancet*, 1961, i, 268.
11. Dundee, J. W. *Brit. J. Anæsth.* 1960, **32**, 450.
12. Cole, W. H. J. *Anæsthesia*, 1955, **10**, 46.
13. Cobb, S. *Anesthesiology*, 1961, **22**, 314.
14. Henrie, J. R., Parkhouse, J., Bickford, R. C. *ibid.* p. 247.
15. McAleavy, J. C., Way, W. L., Alstatt, A. H., Gualagni, N. P., Severinghaus, J. W. *ibid.* p. 260.
16. Clutton-Brock, J. *Brit. J. Anæsth.* 1961, **33**, 214.
17. Page, I. H. *Amer. J. med. Sci.* 1935, **190**, 9.
18. Bennett, I. L., Jr., Heyman, A. *Amer. J. Med.* 1948, **5**, 729.
19. Penfield, W. *Arch. Neurol. Psychiat.* 1929, **22**, 358.
20. Montgomery, B. M. *Arch. intern. Med.* 1961, **108**, 559.

NAPOLEON'S DEATH

Napoleon Buonaparte was the first man about whose adult private life we know almost everything: everything, that is, except the cause of his death. In his lifetime he was recognised universally as a nonesuch: his privacy was not protected by the sacred aura of an ancient crown; his habits were not such as to win the loyalty of all the members of his entourage. In health and sickness his actions and reactions were recorded again and again. We know least about the last three years of his life, because by then most of his memorialists had, willy nilly, left St. Helena and the one who remained was, although a physician, an abandoned liar.[22] Since his surrender Buonaparte's health had fluctuated; sometimes he rode and gardened and talked with vigour and enthusiasm, at other times he sank into querulous torpor. Medical attendants came and went, and their accounts of his symptoms are so diverse as to justify any or all of the diagnoses which have been made retrospectively.[23] After some bouts of violent abdominal pain he died slowly and apparently in peace. He himself had asked that a necropsy should be made, and for once Sir Hudson Lowe was only too glad to comply with his request. The examination was made in the presence of six British doctors by Francesco Antommarchi, his Corsican physician, who had studied anatomy under the great Mascagni. It was probably performed as well as might be expected at that time, but the findings were in dispute. There was general agreement that the stomach contained altered blood and that the pyloric part was ulcerated. No secondary deposits were detected. Dr. Shortt, the principal medical officer of the island, thought that the liver was abnormally large and hard, but on the orders of the Governor this sentence was struck out of the report. This gave " cancer of the stomach " as the cause of death and was signed by the P.M.O. and four other doctors—one of whom had not seen the necropsy. Antommarchi refused to sign. On his return to Europe he produced his own report (together with some pathological specimens) and four years later a second (and contradictory) version.

Taking into account the clinical history of his later

21. Millikan, C. H., Sickert, R. G. *Proc. Mayo Clin.* 1955, **30**, 61.
22. Lord Roseberry. Napoleon: The Last Phase. London, 1900.
23. Keith, A. Hunterian lecture, 1913.

years and the arguments at the postmortem table, it is a reasonable guess that for some time Buonaparte had suffered from an affliction of the liver. No organ is more fruitful of suppositious diagnoses, and all the obvious possibilities have been advanced at one time or another. One of these is arsenical poisoning—a useful stick with which to beat perfidious Albion—and this gained some support when the body was found in good preservation when it was returned to France in 1840. Quite recently the department of forensic medicine at Glasgow had the opportunity of analysing by a new activation technique a tuft of hair believed to have been removed from the body soon after death.[24] The test showed that the Napoleonic lock had an arsenic content of 10·38 p.p.m., compared with a normal value of about 0·8 p.p.m. Unfortunately there was not enough hair to discover whether the arsenic was distributed evenly along its length.

Assuming that the hair came from Buonaparte's head—and the substitution of relics, both holy and medicolegal, is not unknown—chronic arsenical poisoning with acute exacerbations must be considered seriously as a cause of death. The symptoms are suggestive—and equally suggestive of beriberi. If Buonaparte died of arsenical poisoning it might have been accidental or homicidal. It seems that St. Helena had a bad name for inducing hepatic disorders. Did some of the springs—including that which supplied Longwood—give water with a high content of arsenic? Rats were the curse of the island. What rat poison—if any—was used in 1821? We can think of cooking-pots made of metal containing arsenic and all the other examples in the books on forensic medicine, but no other inmate of Longwood seems to have had a similar disease. This was the heroic age of therapeutics, and it may be that for years his doctors had been overgenerous with their arsenical tonics. As to deliberate poisoning, there were so many inhabitants of St. Helena who would have welcomed his death that it is hard to know where to start the guessing-game. There were two brigs and two frigates on guard duty and three battalions of infantry on shore. Everyone from colonels to camp followers would have liked to leave this gloomy rock where they were imprisoned as securely as their one prisoner. Sir Hudson Lowe was a stupid man and rather an unpleasant one, but his whole history suggests the bureaucrat who would do nothing without the backing of "higher authority". Higher authority was Lord Bathurst. He was no genius; but poisoning for political ends is not, as far as we know, a common habit among English noblemen. The most damning evidence against the Governor was his anxiety to suppress the postmortem evidence of an enlarged liver. His task was twofold: to keep Buonaparte alive, and to prevent his escape. Neither he nor Lord Bathurst nor the Cabinet were anxious to face the accusation that they had gaoled the most valuable prisoner in the world in a place where he was likely to die of the endemic hepatitis—whatever this may have been. It is far more difficult to suspect any of the French attendants. They had sacrificed everything to serve the man whom they adored. They were, as might be expected, an odd lot and they quarrelled among themselves like children. Who knows to what lengths venom or misguided loyalty might have persuaded a mind already unsettled by long confinement in close company? (Antommarchi, an obvious suspect, is excused because the symptoms had begun before he arrived.) Or was it a self-inflicted injury? On St. Helena the Emperor had only one wish—to get away. His courage soared and lapsed. In the times of depression the thought must have come to him that if he could not return to power and glory he could at least ruin his oppressor, Sir Hudson Lowe—and in this he was entirely successful. Writers of detective fiction can begin at this point.

24. Forshufvud, S., Smith, H., Wassen, A. *Nature, Lond.* 1961, **192**, 103.

MECHANISMS OF HEARTBURN

HEARTBURN is a burning sensation, felt behind the sternum and rising to the throat, often associated with regurgitation of acid or bitter fluid into the back of the mouth.[1] The cause of this familiar symptom is undecided. We do know that heartburn can be reproduced in normal people by distension of a balloon in the lower œsophagus.[2] such a mechanism may explain the pain as beer is gulped too fast, but it must be rare outside the pub.

A careful study of the mechanism of heartburn has been reported by Tuttle et al.[3] in Los Angeles. Simultaneous records of œsophageal and gastric pressures, and of pH in the lower œsophagus, were made in patients with œsophagitis during spontaneous and induced heartburn. Perfusion of the lower œsophagus with *N*/10 hydrochloric acid (HCl) produced heartburn, together with a distinct fall in œsophageal pH. Equal volumes of physiological saline solution, sucrose solution, and more dilute HCl did not produce heartburn. The œsophageal pressure rose slightly and equally with all solutions, indicating that HCl produced heartburn by changing acidity without increasing muscular activity. These patients were studied in the sitting position. Other workers [4] have reported significant increases in œsophageal pressure during acid perfusion in recumbency; and in this position, when heartburn usually develops, fluid accumulation with distension may well cause increased muscular activity. HCl is known to cause heartburn only in the presence of œsophagitis,[2] and heartburn can occur in association with achlorhydria [5]; so œsophageal acidity is not alone responsible.

More transient heartburn was observed by Tuttle et al. in association with belching; the œsophageal pressure rose sharply, but there were no consistent pH changes. Belching was found to be a stronger stimulus in provoking heartburn; as œsophagitis improved, pain developed with belching, but not with HCl, and finally neither induced heartburn. Disordered œsophageal mobility was rarely associated with gastro-œsophageal reflux; of 209 patients with reflux, only 9 had notably increased œsophageal pressures and increased peristalsis of the lower half of the œsophagus.

Increased acidity, therefore, and distension during belching will produce heartburn in an inflamed œsophagus. In the erect position increased muscular activity is rarely responsible, but it may be more important, in association with acid reflux, when the patient is recumbent.

Mr. E. C. BEVERS, consulting surgeon to the Radcliffe Infirmary, Oxford, and a former chairman of the United Oxford Hospitals, died on Dec. 13.

Sir HENRY DALE has resigned from the chairmanship of the governing body of the Lister Institute of Preventive Medicine, which he has held for twenty years. He is now succeeded by Sir CHARLES DODDS.

1. See *Lancet*, 1960, ii, 1397.
2. Jones, C. M. Digestive Tract Pain. New York, 1938.
3. Tuttle, S. G., Rufin, F., Bettarello, A. *Ann. intern. Med.* 1961, **55**, 292.
4. Siegel, C. I., Hendrix, T. R. *Clin. Res.* 1960, **8**, 369.
5. Palmer, E. D. *New Engl. Med. J.* 1960, **262**, 927.

Points of View

REFLECTIONS ON THE SIGNIFICANCE OF MORBIDITY *

STANLEY ALSTEAD
C.B.E., M.D. Lpool, F.R.S.E.

REGIUS PROFESSOR OF MATERIA MEDICA AND THERAPEUTICS IN THE UNIVERSITY OF GLASGOW, PHYSICIAN, STOBHILL GENERAL HOSPITAL, GLASGOW

IT is said that man has taken about 2 million years to evolve to his present state; and if we adopt a familiar unit of time, namely *one day*, to represent the past 100,000 years of man's life on Earth, the duration of an individual's mature existence is less than 2 seconds. If the time clock of man's existence is marked in *millions* of years, then the chronicle of our effective lives is transient in the extreme.

Clearly, however, we are *not* continuously oppressed by the fleeting character of existence: our sense of time is evidently geared to natural phenomena such as the alternation of night and day, the circuit of the seasons, the presence of innumerable animals and plants declaring their various stages of maturation—and by implication the passing of time, and change and decay in a host of inanimate objects that surround us all our lives. We may claim too that man is endowed with awareness of what is called " the present " as well as a capacity for storing the memory of events that are past; he is also able to project his life into the future in the sense that he declares intentions regarding the things that he will do when " the future " has become " the present ".

Each human being is one in 3000 million people now living on the Earth; and on the geologic clock his position in Time and Space makes him but a grain of sand on the seashore. But even so, his sense of reality as an individual is acute. He has not only the capacity to look outwards on the physical states of the world and the Universe, but also a capacity for introspection. Such a stereoscopic mechanism in promoting the sense of awareness is probably the most significant characteristic determining his dominance in the animal kingdom.

CONCEPT OF MORBIDITY

I suggest that when we consider the phenomenon of morbidity, as it affects living organisms in general and the life of man in particular, we should strive for a comprehensive view as well as the egocentric one; that we should take note of the objectivity of the archæologist, the physicist, and the zoologist—thinking of their subjects, as they do, in terms of the geologic time clock. Within my concept of morbidity I would thus include major calamities involving communities or large groups (as distinct from individuals). For example, tidal wave, typhoon, and hurricane—summarily lumped together as Acts of God—have frequently destroyed man and beast in ways that come within the daily experience of the forensic pathologist.

The broad or geologic view of the phenomenon of morbidity is likely to be more rewarding—in the teleological sense—than a detailed examination of the consequences of morbidity in the single organism. Thus it is notoriously difficult for man to derive useful information from studying personal experience of morbidity in its various forms. We begin by asking him to be as objective as possible, but we know that when morbidity exists in human beings it is characterised by disappearance of the state of objectivity in the patient and in observers who stand in some emotional relationship with him—such as close relatives.

The short dimension of individual experience in the course of living has to be distinguished from the long dimension which extends into universal morbidity in all living things from the beginning of plant and animal life on the Earth. Although I am urging that we should try to visualise experience of morbidity graphically in rectangular pattern with a long geologic arm and a short personal one, I admit that it is extremely difficult to prevent approximation—and even overlapping—of these components. The personal or individualistic experience, despite the absurdity of its limitations, is like cheerfulness: " it keeps breaking through ".

STABILITY OF THE LONG DIMENSION

Despite the bias we all feel towards opinions based on personal experience, I believe that the most valuable lessons will probably be learnt from the study of this " long dimension " of the concept of morbidity. This has certainly been true in the case of the study of the basic sciences, and in particular in the development of our understanding of natural philosophy. Here it is obvious that when man has become excessively egocentric—seeking to emphasise his own significance and the importance of his own planet—he has made some of his most serious errors; and these, multiplying like weeds, have brought with them intellectual chaos, superstition, and fear.

IRREMEDIABLE MORBIDITY

One important phenomenon in the long-term history of morbidity is so obvious that we tend to ignore it. I mean that some of the principal sources of morbidity and its attendant " suffering " are mingled inextricably with physiology as distinct from pathology. I refer, of course, to the abnormalities apparent at birth or later which are determined genetically. There is also the morbidity occurring in mothers and infants during parturition and attributable to purely mechanical circumstances summed up in the word " disproportion ". Again, if we move to the other end of the lifespan of man and all other living organisms, we witness the onset of morbidity in terms of the disabilities characteristic (as we say) of senescence and senility. Death of the organism is eventually inevitable, and morbidity of some degree is the prelude to death. It may be urged that much can be done to alleviate the effects of morbidity in senile people. This is true but it is wholly irrelevant: what we are concerned with is the undoubted association of morbidity with the expected phase of senescence that heralds death.

Thus, in the fœtus unborn, in the infant at birth, and also in the months preceding death in old age we identify this state of morbidity. Further, in this context and looked at both historically and on a worldwide scale, these are manifestations of morbidity over which we have no powers to intervene.

I am not arguing against the possibility of intervention to alleviate suffering or to prevent calamity. For example, the obstetrician may justifiably and successfully intervene in a case of difficult labour. His action, however, does not detract from the truth of the statements I have made. I am concerned just now with the factual situation—the *existence* of a morbid state in relation to the physiological

* From a presidential address to the Royal Medico-Chirurgical Society of Glasgow, October, 1961.

process of childbirth. Indeed, by his decision to intervene the obstetrician is declaring his conviction that this " physiological " situation is fraught with possible calamity for the mother and the infant.

Let us turn our attention to another source of morbidity which from time immemorial has grown into a veritable ocean of calamity and suffering: I refer to bodily injuries of all kinds. It might be thought that once a man or an animal had been launched in safety and in health on the surface of the Earth he would be granted freedom from physical hazards during the prime of life. Not so: in every phase of his existence, in every set of circumstances which denote a particular way of living, in every part of the world in which he lives—there are dangers that cost life and limb.

When I referred to morbidity within the realm of physiology I was in effect suggesting the inevitability of morbidity. When we consider the morbidity associated with trauma, the case for inevitability is not so strong. Is it inevitable that in Britain alone about 50 human lives should be terminated every day from accidents in home and workshop and on the roads? It can be argued that much of this morbidity could be prevented; that many lives are thrown away through sheer stupidity or selfishness or simple errors of judgment. Such comment merely invites us to give a sufficiently comprehensive definition of an accident, but I shall refrain from pursuing the theme. Suffice it to say that a great deal of traumatic morbidity is related to circumstances in which the victim was entirely blameless; or is even the outcome of what we regard as virtuous conduct—such as attempts at rescue at sea or in the mines.

But let us beware of taking too domestic a view of this kind of morbidity. All that I am trying to take from this facet of the phenomenon of morbidity is this: a substantial proportion of it is fortuitous in the sense that it is beyond the control of man.

ATTITUDES TO MORBIDITY

How has it come about that in the face of strong evidence to the contrary civilised man has rejected the assertion that experience of morbidity is an integral and intended part of life? How can we, as we look back at what has happened to man and animals over a period of a mere 10,000 years, suppose that the circumstances *we call* disease and morbidity were not intended to occur? Here are circumstances that cry out for a new orientation of the human attitude. The facts of morbidity may be unpleasant when we approach them as we do today. But it may well be that the " fault " is not in the factual situation: it may reside in some temporary perversity of human thought.

MEDICAL PROGRESS

I have another plea to make: it is for a more realistic and more modest attitude towards what man has accomplished in our own day in the realm of medical practice. We hear a great deal about advances in the principles of hygiene, new techniques in diagnosis, ingenious methods for meeting the difficulties of surgical operation on vital structures, and the creation of new chemical compounds as specifics against microbial and parasitic diseases which afflict man and the higher animals. Against this background displaying human intelligence and inventiveness, we are apt to dilate upon the increased expectation of life in civilised communities such as our own.

It is much more important, however, that we should take stock of these developments against a background infinitely greater than that defined by our textbooks and our professional journals. When we preen ourselves on the increased life span of people in western Europe in the past few decades, let us remember at least three things. First, that we are only just beginning to realise the importance of passing on this boon (such as it is) to the thousands of millions who live beyond the Western world. Secondly, this increase in duration of the individual human life carries with it a disproportionate increase in the burden of morbidity to be borne by the individual and by the community. Thirdly, the increased expectation of life in a relatively small proportion of mankind—though adding something to the total of existing human life—fades into insignificance by comparison with the increase brought about by the rising birth-rate in most parts of the world and the rising curve of world population. What one might call the " biological momentum " of this enormous mass of living matter is something which defies measurement by the mind of man: we cannot even imagine a suitable yardstick for the job. We can merely declare that it is so immense and so persistive that the concept of biological momentum strongly reinforces our ideas of purposiveness.

DISCOVERY AND MORBIDITY

Our valuable discoveries in the prevention and treatment of disease—how much impact have they made on mankind as a whole? More than half of the world's population is undernourished; and our vaunted specific remedies have still to find their way to our 10 million lepers, the 50 million suffering from yaws, the 300 million disabled by elephantiasis, the 400 million more or less blinded by trachoma, and the many hundreds of millions whose lives are still blighted by malaria—and of whom about one million die every year. This is the picture of morbidity in which your life and mine are set. It is a legacy created out of living things a long way back on the geologic time-scale. Is it likely that it will change overnight—by which I mean change in relation to the short arm of measurement which is the lifespan of a man?

The discovery of the sulphonamides and the utilisation of antibiotics for therapeutic purposes were events greeted with worldwide acclamation. I am in no doubt that as exhibitions of human ingenuity, and as proofs of the intellectual power that resides in the brain of man, these events are highly significant. And in research in medical science many other examples could be cited, such as provision of clean food and water, the concept of antiseptic surgery, our insight into the pathogenesis of the anæmias, and the synthesis of vitamin B_{12}—to mention at random a few out of scores of similar achievements. All these developments are on record. What is their bearing on the phenomenon of morbidity considered in its total sense—throughout the world and also against the geologic background? If we detach ourselves from our domestic moorings and move into the main stream of Space Time in relation to our Earth and our Universe we shall be forced to regard these events as relatively microscopic. They can be compared with the effect of placing a stone about the size of a cricket ball on the bed of a long and mighty river: such is their power to deflect the flow of events in the material world that includes the animate as well as the inanimate.

INSULATION FROM MORBIDITY

We are all reconciled to the fact that physical death comes to every living organism. What we are not

reconciled to is the manner in which death occurs and the fact that it so often takes place prematurely; for man has set up standards which determine what he considers he is entitled to expect. But the standards used have only local and contemporary significance. To be of any real value they should be a great deal more comprehensive—taking into account patterns of life and the incidence of morbidity applicable to all mankind and over a very long period of time.

In practice we almost completely ignore such considerations: we can develop—and indeed we do develop—a remarkable degree of imperturbability about the morbidity and the expectation of life that existed say in medieval England or among the tens of thousands of destitute people in Hong Kong just now; and when we hear of disaster overtaking a ship or an aircraft and the sudden extinction of a hundred lives, how long do the thoughts of the calamity remain in our minds? Individually we are able to note morbidity—even on a massive scale—and yet to be scarcely affected by it so long as it is sufficiently remote from us.

If there is design and purpose in psychological as well as physical mechanisms, this state of insulation that protects our minds from the excessive impact of morbidity in the world is presumably intended for our welfare—to prevent too great a sense of oppression and despondency that might overwhelm us. I do not, of course, suggest that we are entirely unaffected by the impact of morbidity—however remote the incidents may be. On the contrary, it would appear that we are intended to be affected by all that we discover—in this field and in many others—but only to an extent that promotes the master plan for the gradual emergence of the spirit of man in its ultimate and perfect form; and I am prepared to believe that what we call morbidity constitutes a vast and heterogeneous group of stimuli which have their part to play in this process.

MORBIDITY AND EVOLUTION

In saying this I am assuming that it is possible for man to make adjustments in his way of thinking—and in particular in his attitude to the significance of morbidity. If we do not allow that within certain limits man—as we know him now—can choose, then discussion of man's attitude to morbidity is futile; for it is obvious that in such circumstances even our judgments would become foregone conclusions. I myself accept the concept of free-will—though of a limited kind and conditioned by circumstances related to man's evolutionary state at any moment in the record of his existence. I therefore feel entitled to urge a more critical attitude in what we accept from religion regarding the phenomenon of morbidity.

Briefly, I reject any doctrine which declares that *morbidity was not intended* or that it emanates from an independent source—a separate creative force which is inherently evil. I am much more disposed to regard all phenomena as belonging to a pattern deliberately contrived.

There are clearly relationships between different species of living organisms, and the nature of these relationships—often enough examples of the genesis of morbidity—is explored systematically in the sciences we call bacteriology, parasitology, virology, mycology, and other studies. In the midst of this extensive field of academic exploration there are indications that man has acquired an attitude of resentment towards the phenomenon of inter-relationships. Thus there are many who regard this matter as a fault in the fabric of Creation and consequently seek relief in the language of the cynic; others see no alternative to an attitude of stoicism or fatalism; and in the principal religious dogmas the apparent difficulty is accepted as a mystery that calls for the exhibition of courage and faith—qualities which are sedulously differentiated from fatalism.

For myself, I believe that we can deliberately set out to alter the existing circumstances (in our context, in terms of health and disease); but such distortions are likely to be very slight in degree, very restricted in their area of operation, and only momentary in their duration—in relation to geologic backgrounds. Nevertheless they may be highly important to us as individuals—acutely concerned with the day-to-day calibrations which are marked on the human component that defines existence.

Special Articles

CHANGE OF DIET OF YEMENITE JEWS IN RELATION TO DIABETES AND ISCHÆMIC HEART-DISEASE

A. M. COHEN
M.D. Beirut
LECTURER IN MEDICINE, DEPARTMENT OF INTERNAL MEDICINE B., ROTHSCHILD HADASSAH UNIVERSITY HOSPITAL, JERUSALEM

SARAH BAVLY
Ph.D. Columbia, N.Y., Dr.Chem. Amsterdam
DEAN, COLLEGE OF NUTRITION AND HOME ECONOMICS, JERUSALEM

RACHEL POZNANSKI
M.S. Lvov
OF THE COLLEGE OF NUTRITION AND HOME ECONOMICS, JERUSALEM

A SIGNIFICANTLY greater prevalence of diabetes mellitus (Cohen 1959, 1961), ischæmic heart-disease (Dreyfus 1953, Toor et al. 1960, Cohen et al. 1960), hypertension (Cohen et al. 1960), and raised plasma cholesterol and β-lipoproteins (Toor et al. 1954, 1957, Brunner et al. 1955) has been found among the settled Yemenites who have lived in Israel for over twenty-five years, or were born in Israel, than in the newcomer Yemenites who have been in the country for less than ten years.

Some epidemiological studies have shown that populations with high rates of ischæmic heart-disease have diets rich in fats and animal proteins (Keys 1953, 1957, Keys et al. 1958, Bronte-Stewart 1958, Olson 1960). Other studies have suggested that other factors are equally closely related to the incidence of ischæmic heart-disease (Yudkin 1957, Yerushalmy and Hilleboe 1957). Since most of these studies were based on comparison of different groups of people, many factors may be responsible for the differences between them (Groen et al. 1952, Dock 1960).

We have investigated the pattern of the diet of Yemenite Jews as eaten in the Yemen and as eaten in Israel, so as to ascertain whether the greater prevalence of diabetes and ischæmic heart-disease after living in Israel can be attributed to a change of diet.

Methods

As a preliminary, we questioned hundreds of Yemenites on their food habits while in the Yemen, and we found two striking features in the previous diet mentioned by all of them. First, the main source of fat in the Yemen had been mutton fat, beef fat, and *Samne* (i.e., butter preserved by evaporation of its water content). Secondly, the quantity of sugar used in the Yemen had been negligible.

COMPARISON OF BUTTER AND SAMNE

—	Butter	*Samne*
Iodine value	32	36
Acidity value	0·9	0·6
Reichert value (sol. Vol. acid)	21·4	29·4
Polensky value (Insol. Vol. acid)	2·0	2·2
Water	11·3	0

A more detailed quantitative survey was then made with the help of a dietitian who herself had arrived from Yemen about ten years ago but was trained in Israel, and who was accordingly familiar with food habits both in the Yemen and in Israel.

The families surveyed consisted of two groups:

(*a*) 20 Yemenite families in Rosh Haayin and Kiryat Yearim, who had lived in Israel for less than ten years, were questioned on their diet in the Yemen. This was done by the "recollection" method—i.e., reconstructing the quantities of food cooked by the housewife, checking the volume of the utensils they brought over with them, and checking these quantities with the husband, who used to purchase the materials. This was helpful, since in this group the husband is the only one who purchases all the food. Thus we had as good a control as possible on the estimates of the housewife.

(*b*) 20 Yemenite families in Jerusalem, who had lived in Israel for more than twenty-five years, were surveyed on their present diet. They were visited daily during a whole week by the dietitian, who noted down daily quantities consumed by the families according to a detailed questionnaire. For this purpose daily and weekly purchases were checked against the daily menus and the amounts found in provisions and cooked dishes in the kitchen. These food quantities were converted into nutrients by using the Food Composition Tables published by the nutrition department of the Israeli Ministry of Education:

Fats were subdivided into: (1) fat of animal origin; (2) margarine, and (3) oil, which in Israel during the last ten years has consisted of soya, sesame, and olive oil.

Proteins were subdivided into animal and vegetable.

Carbohydrates were subclassified under: (1) total carbohydrates; (2) sucrose; and (3) monosaccharide and disaccharide, which includes carbohydrates from fruits, vegetables, milk, honey, and sucrose.

Waste was taken into account by using the "as purchased" content of the foods.

We compared these two groups with a further two groups derived from another survey * of weighed food intake:

(*c*) 12 Yemenite families from Rosh Haayin who had lived in Israel for less than ten years.

(*d*) 19 families of European origin who had lived in Israel for more than ten years.

The weight and height of unselected 210 new immigrants (100 males and 110 females) and 235 settled Yemenites (110 males and 125 females) over 30 years of age were compared with mean weight of same height, age, and sex, according to the Actuarial Society of America, 1912 (table II).

* Bavly, S. Food Consumption and Levels of Nutrition of the Rural Population in Israel (to be published).

TABLE II—COMPARATIVE WEIGHT OF 210 (100 MALE AND 110 FEMALE) NEW IMMIGRANTS AND 235 (110 MALE AND 125 FEMALE) OLD SETTLED YEMENITES OVER 30 YEARS OF AGE

Comparative weight is the weight of the Yemenite minus the mean weight of whites of the same height, age, and sex. Individuals over 55 years were compared with the mean given for 55 years, according to Actuarial Society of America, 1912.

—	Males		Females	
	Immigrants	Settlers	Immigrants	Settlers
Mean weight (kg.)	56·5 ± 9·1	64·9 ± 13·2	51·3 ± 9·1	59·5 ± 9·8
Mean height (cm.)	162·2 ± 6·5	160·3 ± 7·6	147·6 ± 6·9	150·3 ± 7·3

Results

The intakes of nutrients are given in table I. Compared with their diet in the Yemen, the diet of Yemenites after long residence in Israel shows:

1. Slightly increased consumption in total calories. This is reflected in increase in average body-weight (table II).

2. No change in total protein.

3. In Yemen, fats were mainly or solely of animal origin—*samne*, milk, mutton, beef, and very few eggs. Vegetable oil was rarely used, and then only for baking special bread for the sabbath and festivals. The amount of fat from animal sources consumed in Yemen was similar to the amount of animal fat plus margarine consumed by the old settled Yemenites in Israel. In Israel there is some increase in oil consumption and consequently in total fat.

4. The carbohydrates consumed in the Yemen were mainly or solely starch—almost no sugar was consumed. In Israel there is a striking increase in sugar consumption, though little increase in total carbohydrates.

5. The above differences hold true also when comparing the diet of the Yemen with the diet of settlers of European origin.

Discussion

Data about the diet in the Yemen are based on recollection, and their accuracy is therefore questionable. But, besides the groups mentioned, we asked hundreds of Yemenites about their diet in Yemen and received consistent answers. Since precautions were taken for checking the data with different members of the family, it may be assumed that the results are reliable qualitatively, and approximately quantitatively.

The fat consumed by Yemenites in Yemen was mainly of animal origin, and in quantity it was not much less than the animal fat plus margarine consumed by the settled Yemenites and settlers of European origin. Yemenites and European settlers in Israel consume far more oil (soya, sesame, and olive) than the Yemenites in Yemen. Thus neither the fat content of the diet nor its nature explains why ischæmic heart-disease and diabetes are relatively rare in the Yemenite new immigrants.

Toor et al. (1954) studied in 1953–54 the diets in 55 families recently arrived from Yemen, and their figure for the fat content was 30 g. per head per day. They did not

TABLE I—FOOD INTAKE PER HEAD PER DAY

Groups	No. of families	No. of adults (15 yr. +)	No. of children (15 yr. −)	Calories	Protein		Fat				Carbohydrates			
					Total (g.)	Animal (%)	Animal (g.)	Animal + margarine (g.)	Oil (g.)	Oil (% of total fat)	Total (g.)	Mono- and di-saccharides (g.)	Sucrose (g.)	% of total calories
Yemenites, in Yemen	20	108	48	2237 ± 98†	80 ± 3·6	37	43 ± 5	43 ± 5	14 ± 2	25	343 ± 15	26	6·6 ± 1	58
Yemenites, old settlers	20	112	66	2559 ± 118	86 ± 4·4	46	42 ± 7	51 ± 3	30 ± 3	37	377 ± 21	154	63 ± 6	59
Yemenites, new immigrants	12	64	38	2468 ± 120	78 ± 4	49	43 ± 8	49 ± 12	21 ± 2	33	373 ± 30	76	51 ± 3	56
Europeans, old settlers	19	62	50	2648 ± 171	80 ± 4	47	40 ± 6	52 ± 4	32 ± 4	38	389 ± 32	167	92 ± 2	59

† Standard deviation of the mean.

specify the proportion of fat derived from animal sources. Our figure for fat in the diet in Yemen is 43 g. The difference may be explained by the fact that when Toor et al. made their study Israel was on rations, and the fat supplied was mainly margarine: not being used to it, the new immigrants may have consumed less than their rations. Their figures for the old settlers, as well as those of Brunner and Loebl (1958), are much the same as ours as regards both quantity and quality. Our results do not support the conclusion reached by these groups of workers that the amount or the nature of the fat consumed by the Yemenites in Yemen protected them against ischæmic heart-disease. Their conclusion is the more difficult to maintain because of the high percentage of oil in the fat in the diet of settled Yemenites, in contrast to the Yemenites in Yemen who consumed very little oil.

The total caloric intake was less in Yemen than in Israel, and this is reflected in the increase in body-weight in the old settled Yemenites (table II). However, the diet in Yemen was by no means a starvation or semi-starvation diet, and cannot account for the low cholesterol and β-lipoprotein found in this group (Portman and Stare 1959); nor is the Yemenites' diet in Israel an excessive one.

One of the main characteristics of the Yemenite diet is the *hilbe* (*Trigonella fenugracum*) which they use as a spice in all their meals. This might be protective against diabetes and ischæmic heart-disease in the newcomer Yemenites; but our study shows that the amount of *hilbe* and other spices consumed by the old settled Yemenites is no different from that consumed in the Yemen.

A significant change in the diet after arriving in Israel is that far more sugar is consumed, though the percentage of calories derived from carbohydrates is little higher. Emerson and Larimore (1924) showed a close relation between sucrose consumption and the death-rate from diabetes in the United States, Great Britain, and Paris. Mills (1930) did not agree with Emerson and Larimore, but said: " It is to be noted, however, that of the thirteen countries highest in consumption of sugar, eleven are found among the thirteen highest in death rate from diabetes." Yudkin (1957) has found that " there is a better relationship with intake of sugar than with any other nutrient we have examined in the relationship between diet and the incidence of death from coronary disease " in different countries. Sucrose has been found to cause an increase in the plasma-cholesterol and β-lipoproteins in animals (Portman et al. 1956, Harper and Elvehjem 1957, Poliack 1958). If a nutrient is an ætiological factor in ischæmic heart-disease or diabetes, the increased consumption of sucrose *might* be responsible, directly or indirectly, for the higher prevalence of these diseases in those Yemenites who have lived for many years in Israel.

Summary

The diet of Yemenite Jews in the Yemen has been compared, retrospectively, with the diet of Yemenite Jews long resident in Israel.

Fat consumed in the Yemen was mainly of animal origin. Its quantity was much the same as the total of animal fat and margarine consumed by Jews living in Israel; but Yemenites in Israel consume daily, in addition, about 16 g. of oil, derived from soya, olive, and sesame. Thus neither the nature nor the amount of fat consumed by the Yemenite Jews in Yemen provides an obvious explanation of why they have less diabetes and ischæmic heart-disease than the Yemenites who have been long settled in Israel.

There is no significant difference in the amount of protein consumed in the Yemen and in Israel.

Whereas in the Yemen no sugar was eaten, about 20% of the carbohydrates taken by Yemenites in Israel is in the form of sucrose. No difference was apparent in the total amount of carbohydrates consumed or in the proportion of calories derived from carbohydrates.

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THE BRIDGMAN UNIT FOR MOTHERS AND BABIES

J. K. RUSSELL
M.D. Aberd., F.R.C.O.G.

PROFESSOR OF OBSTETRICS AND GYNÆCOLOGY, UNIVERSITY OF DURHAM; CONSULTANT OBSTETRICIAN, PRINCESS MARY MATERNITY HOSPITAL, NEWCASTLE UPON TYNE

WHEN confinement takes place at home it is customary and natural for the baby to be nursed in a cot by the mother's bed, and a similar arrangement is found in many hospitals. On the other hand both in Britain and on the Continent it is not unusual to find maternity units where babies are kept separate from their mothers in ward nurseries.

In hospital practice neither arrangement is satisfactory. The " rooming-in " system has a strong emotional appeal, but babies nursed in open wards are exposed to a variety of infections. A procession of people pass through the ward each day—doctors, nurses, students, physiotherapists, Red Cross workers, cleaners, visitors and others. All this movement creates dust which is liable to contain resistant staphylococci. Equally important are the respiratory infections brought into the wards by these people. Some control can be exercised over hospital staff, but it is quite impossible to maintain an effective check on patients' visitors. The busy ward is not a favourable environment for the newborn baby.

The puerperal mother needs rest and quiet but is unlikely to get this in a busy lying-in ward. Constant interruption and noise are disturbing features of our hospitals, and common causes of complaint.[1 2] With four,

1. *Times*, March 23, 1960.
2. Pattern of the In-patient's Day. H.M. Stationery Office, 1961.

six, or eight mothers and their babies in a ward there can be little rest for the mothers; by the time nurse has removed a crying baby during the night or at rest-time the whole ward may be awake. It is not surprising that some mothers are tired and emotionally upset when they leave hospital, and welcome the quiet of their own homes.

The communal nursery for twenty or thirty babies has its own disadvantages and dangers. It is unnatural for the baby to be out of the mother's sight for most of the day, and this can only lead to tension and anxiety; circumstances are against the young mother learning how to handle and care for her baby and a valuable opportunity for health education is lost. The danger of epidemic neonatal infection is always present in hospital, and the chance of this happening is probably greater when babies are kept in communal nurseries.[3]

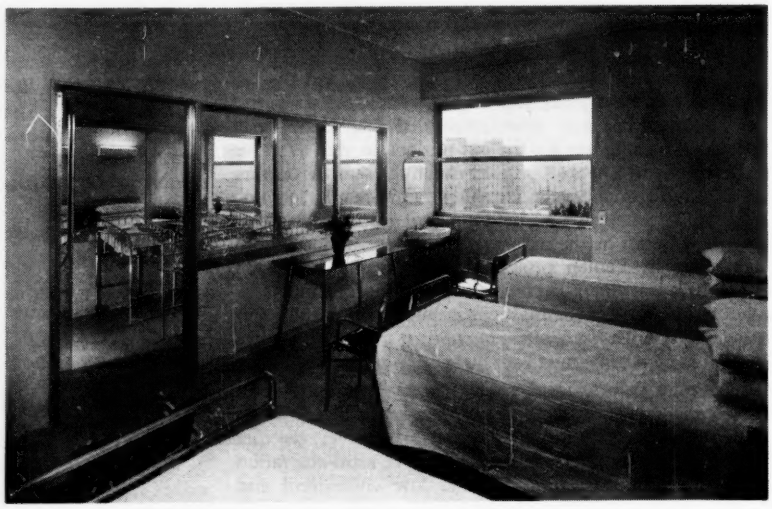

Fig. 2—Six-bed unit for mothers and babies in the Sesto San Giovanni Hospital, Milan: view from one ward through the nursery to the second mothers' ward.

Many Continental clinics have adopted a compromise between " rooming-in " and the ward communal nursery, and Dr. R. F. Bridgman's name is generally associated with these units for mothers and babies. I have recently seen examples of the Bridgman unit in Dutch and Italian hospitals, and they are also to be found in French and Belgian maternity clinics. The principle features of a four-bed Bridgman unit are shown in fig. 1. There is direct access to the mothers' wards from the main corridor, but the central nursery between the two wards is open only to mothers and selected members of the hospital staff. In some units the walls of the nursery are of double glass from floor to ceiling (fig. 1); others have large double-glazed windows along the length of the walls and the babies are in full view of the mothers in both wards (fig. 2). Entry to the nursery may be through a single door as in fig. 1; or there may be two doors, one leading in from each of the wards (fig. 2). It is possible to vary the size and design of this type of unit without departing from the basic concept, and the part taken by the mothers in managing their babies is for the medical and nursing staffs to decide. All facilities for the care of babies are provided in the nursery.

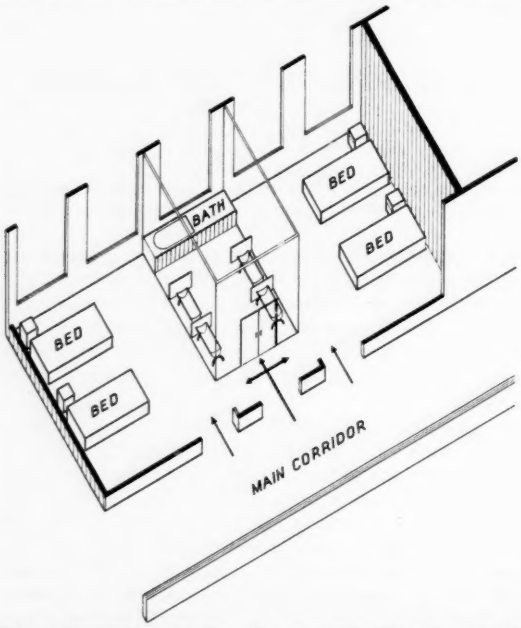

Fig. 1—Isometric projection of a four-bed Bridgman unit for mothers and babies.

According to Dr. Bridgman [4] his design of unit [5 6] is a modification of an arrangement he saw many years ago in the maternity wards of the George Washington Hospital in Washington, D.C. From my inspection of these units and after talking to many of the mothers, I am sure that Dr. Bridgman's plan has the merit of common sense, is popular with mothers, relatives, and staff, and is based on sound scientific principles. The individual mothers have free access to the nursery at all times, but at rest periods or when there are visitors the nursery doors are closed. By day the babies are plainly seen through the glass walls, and at night subdued lighting is switched on in the nursery. Double glazing reduces the amount of noise from crying babies, and during the night a mother can be roused quietly by a nurse to feed her hungry baby without disturbing the other mothers. With fewer visitors and less movement in the nursery the risk of neonatal infection is likely to be reduced.

I should like to express my thanks to Dr. R. F. Bridgman, directeur adjoint de la santé de la Seine, Paris, for his personal comments. My attention was first drawn to the Bridgman unit by Dr. A. Tillema and Dr. J. L. H. Specken, and I am most grateful to them for showing me round the new maternity unit in the St. Joseph Ziekenhuis, Eindhoven. I am also indebted to Dr. A. Chiara and the director of the new Sesto San Giovanni Hospital in Milan for their kindness in showing me over that hospital and for permission to reproduce fig. 2. My thanks are also due to Miss D. Mustart for drawing the isometric projection of a Bridgman unit.

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Very Special Article

APRIORITY

Thoughts on Treatment

RICHARD ASHER
M.D. Lond., F.R.C.P.

ONE of the most important things about treatment is that it should *be* effective—not merely that it *ought* to be effective. A remedy which is known to work, though nobody knows why, is preferable to a remedy which has the support of theory without the confirmation of practice.

J. W. Todd emphasises this, both in his book [1] and in his article on the history of medicine.[2] He applies the words *a priori* to the reasoning behind many traditional treatments. For my part I agree with Gowers [3] that writers could probably get along quite well without *a priori*, and I dislike the expression for I never can remember exactly what it means. I think my difficulty is partly due to its having several subdivisions of meaning.

There is little doubt about its general sense. It describes the arguments, reasoning, speculations, notions, traditions, or other support for conclusions which have not been backed by any kind of practical experiment. When I think of the *a priori* grounds for many of the futile treatments that used to be given and still are given, I realise what many fine shades of apriority [4] there are. I set them out now.

SHADES OF MEANING

1. There are some things we are so sure of that we accept them without proof. They are assumptions which we act on without question and they cannot be proved. The Kantian school of philosophy applied the term *a priori* to reasoning based on such things. They held that such ideas—e.g., that good health is more to be desired than bad, or that doctors should prefer effective treatment to ineffective—are inborn, inherent within the mind, and independent of experience. I prefer to say they are damned obvious. Todd suggests that *a priori* argument is always worthless, but this kind is not. We could not get anywhere without these unprovable assumptions.

2. The witchcraft type of treatment—" eye of newt and toe of frog, wool of bat and tongue of dog "—of which many examples still exist, is based on no reasoning whatever. It is just a wild senseless concoction, with not even an assumed axiom to back it. An example in modern times is the use of a brown mess of powdered fossilised fish mixed with glycerin to spread on erysipelas (glycerin of icthyol is still applied today, although backed by sulphonamides). This sort of nonsense has nothing behind it except sheer imagination, the desire to concoct an elaborate and disgusting potion, and a trace of the two primitive ideas: (a) anything unpleasant is good for you (the inverted Marie Lloyd theme—i.e., " a little of what you don't fancy does you good "); and (b) anything difficult to get, rare, or expensive is more likely to be effective than something cheap, common, and easily got.[5] Todd would call the basis of such mumbojumbo *a priori;* but there hardly seems enough assumption behind it to justify it. Nomenclature is difficult, but perhaps " pure unadulterated boloney " might do (=P.U.B.).

3. Gradually moving up the scale (grading *a priori* treatments according to their boloney content, with the boloney content diminishing as we advance), we come to the higher forms of witchcraft, which are backed by theories which, though patently nonsensical, do contain minute traces of theoretical reasoning. Though the administration of bats' blood for insomnia is pure boloney, I maintain that treating insomnia with the blood of dormice (as has been repeatedly advocated by Asher) [6] is not wholly devoid of reason. Nor for that matter is the practice of fumigating and scrubbing an operating-theatre after a death, rightly condemned by Forbes[7]; because such things, though unsupported by experience, are at least backed by reasoning of a kind—i.e., that there is something in the blood of dormice which promotes sleep, and that the spirit of a patient who has died in the theatre will linger on and be a general nuisance unless exorcised. This sort of nonsense might be called H.B.C. (high boloney content), as opposed to P.U.B., the pure variety described above. Moreover something which was P.U.B. when first advocated by some wild witch, may reach H.B.C. status later because it acquires the backing of a reasonable assumption—i.e., " treatment which is alleged to do good, is more likely to be helpful than treatment which has never been advocated by anyone ". There is obviously some sense in that, although it is an argument that has perpetuated more harmful or useless procedures than any other.

4. Going one grade higher still, we come to forms of treatment which, though not supported by scientific experiment, have a fairly reasonable theoretical basis. I will term this P.C.B. (pretty convincing boloney). I am thinking of ideas such as: that rough or " strong " food will provoke a stomach afflicted by ulcer; that a man whose temperature falls below normal because of shock should be heated up till it becomes normal; and many other such things. Here the arguments are, though *a priori*, of a far more civilised standard than the " eye of newt " variety. Their use by medical men arises from the unsound practice of acting on theoretical grounds without bothering to determine whether experience confirms the theoretical benefits; yet P.C.B. differs from the next grade because, if more care had been spent on the theory, a more effective conclusion might have been reached. For instance, there are *a priori* reasons for *not* heating a shocked person, if you assume that the vital organs benefit from the blood lent to them from the cold and bloodless skin.

5. The most reasonable and forgivable kind of *a priori* treatment is the one where you give a remedy which theoretically ought to work, even though properly controlled clinical trials have not yet been conducted to make sure that it does.

I refer here to treatments based on scientific experiments. For example, if the bacteria grown from a patient are killed in vitro by one antibiotic and not by another, it is plain that the former should be given rather than the latter. I am not sure whether this could be called an *a priori* argument, since the exact limits of the term depend on who uses it. One dictionary definition of *a priori* gives

1. Todd, J W., and others. Text Book of Clinical Medicine. London, 1960.
2. Todd, J. W. *Lancet*, Aug. 26, 1961, p. 480.
3. Gowers, E. P. Complete Plain Words; p. 42. H.M. Stationery Office, 1954.
4. Synthesised by Asher in the Etymologico-Semantic Laboratories, London. (Patent pending.)
5. Field-Marshal Naaman, who had leprosy, heard of a very good skin man through his wife's maid. He went to see him, with encouragement from the King, and arrived, in the equivalent of a Bentley, prepared to pay a large fee for the invocations and laying-on-of-hands which he expected. He was bitterly incensed when he got a simple prescription for river water and was charged nothing. " Sir," his servants pointed out, " if the prophet had asked you to do elaborate things you'd have done them, wouldn't you? Why not try this simple thing? " (Adapted from *II Kings*, chap. v, 1–14.)
6. See *Lancet*, 1943, i, 249; *Middlesex Hosp. J.* February, 1960, p. 9.
7. Forbes, R. *Lancet*, 1946, ii, 293.

" theoretical " as one of its meanings,[8] and the treatment of infections with antibiotics to which they are sensitive in the laboratory is certainly theoretical. Moreover, we all see organisms which are highly sensitive in laboratory tests fail to disappear when the appropriate antibiotic is given; whereas organisms which appear virtually immortal when tested on an agar plate may quickly expire in vivo when exposed to an antibiotic which is theoretically ineffective.

One can call this type of treatment *a priori* because it is theoretical (and not always practical); but if the term is restricted to arguments based on assumed axioms—i.e., *pure* armchair reasoning—then treatment which is supported by scientific *experiments* does not qualify as *a priori*.

Whether we use the word or not, it is certainly true that many things which in theory ought to be highly effective turn out in practice to be completely useless. We could call this type of treatment U.B.C., or uncertain boloney content.

8. Shorter Oxford English Dictionary. London, 1956.

CONCLUSION

The best way to decide which remedies are good is " try them and see ". The question to which we must always find an answer is not " should it work? " but " does it work? " (even if we do not know why).

SUMMARY

Experientia docet.

Public Health

THE PUBLIC'S HEALTH IN 1960

Dr. G. E. Godber, in his first annual report[1] as chief medical officer of the Ministry of Health, refers to the association between bronchial carcinoma and cigarette-smoking in the following words:

" The epidemic of cancer of the lung pursues a predictable course in men with an increase which can only be arrested in the next few years by a general change in smoking habits—in fact by stopping or markedly reducing the excessive smoking of cigarettes. There are signs, too, of the increase which women must be expected to suffer at the appropriate interval after their increased use of cigarettes began. There are certainly other factors in the causation of lung cancer, but only this is under our control. The public does not lack information; it appears to lack conviction or the will."

Last year, of all cancer deaths in men, 18,882, representing 35·8% of the total, were attributed to lung cancer. This is 701 (3·9%) more than in 1959—the smallest percentage increase since 1950. Female lung-cancer deaths numbered 3118. This is an increase of 236 (8·2%) over the total in the previous year.

MARRIAGE AND CHILDBEARING

Since 1946 the average age of spinsters marrying bachelors has fallen from 24·16 years to 22. Of all marriages, 9% now take place while the bride and bridgroom are both under 21 (this compares with 4% in 1946). Besides marrying earlier, married couples have shortened the time within which they have their children. The proportion of the total family which is achieved in the first five years of marriage increased from 50·0% for marriages of 1938 to 54·3% for marriages of 1950.

In 1960 there were 785,005 live births, representing a birth-rate of 17·2 per 1000 population.

INFANT MORTALITY

In 1960 the infant-mortality rate was 21·8 per 1000 live births. This is the lowest figure recorded in this country and compares with a rate of 22·2 in 1959. The neonatal mortality-rate continues to decline slowly (15·5 per 1000 live births); but the postneonatal infant-mortality rate is levelling out, and it should not be thought that further reduction in this rate is impossible: "One has only to compare the figure of 6·3 per thousand live births for England and Wales in 1960 with that of 2·7 per thousand for Sweden in 1960."

FOOD-POISONING

Altogether 6428 food-poisoning incidents were reported through the laboratory services—a reduction of 18% on the number in 1959 and of 12% on that in 1958. The reduction was particularly great in incidents due to salmonellæ other than *Salm. typhimurium*. The foods associated with 165 outbreaks were identified. 137 (83% of the total known to have been associated with a specified article of food) were associated with processed and made-up meat dishes. This group included 15 outbreaks of the toxin type, due to *Staphylococcus aureus*, associated with consumption of corned beef canned in two different countries by the same firm.

1. Report of the Ministry of Health for the year 1960. Part II: On the State of the Public Health. H.M. Stationery Office. 1961. Pp. 239.

VENEREAL DISEASES

In 1960 the number of cases of early syphilis (infections of less than one year) treated at the clinics were: in males, 819 (564 in 1959); and in females, 175 (209 in 1959). It is still not certain, says the report, that the increase in males indicates the upward trend which for the past few years has caused anxiety in other countries. The slight reduction in cases in females suggests difficulty in tracing infectious contacts rather than diminished incidence. The number of new cases of gonorrhœa diagnosed at the clinics rose from 31,344 in 1959 (when the total was the highest since 1947) to 33,770 in 1960. The rate of increase is apparently slowing: in 1960 it amounted to 8%, compared with 12% in 1959. In patients harbouring partially resistant strains, treatment may suppress symptoms without achieving cure; and if these patients assume that they are cured and discontinue attendance, as they are apt to do, the infection may be spread.

HOSPITAL DESIGN

The report includes an informative section on principles in the planning of hospitals.

The diagnostic and treatment departments in general should be centralised, to serve the whole hospital. Outpatient facilities should provide for any patient who can be satisfactorily treated without admission. This conception has led to the planning of facilities for day patients in outpatient departments. These facilities will include operating-theatres and treatment rooms together with associated sitting-rooms and day beds. Such beds might possibly also be used for overnight stay—for example, by a patient, living at a distance, on the night before he is due to attend for investigation or treatment.

Wherever possible the key diagnostic and treatment departments, the casualty department, and the outpatient department should be sited in single-storey buildings or, where this is impracticable, in low buildings, to ensure space for expansion and to allow for the free planning of each individual department.

The diagnostic departments need to be readily accessible from the wards; and one way of arranging this (as at the new hospital to be built at Wexham Park, Slough) is for the ward units to be planned as a series of single-storey buildings, leading off a main corridor which provides a link to the diagnostic departments. The single-storey hospital has the great advantages that it can be extended and that it does not rely on lifts. But single-storey construction is unsuitable for large hospitals; and where a multi-storey building is necessary, one of the most satisfactory solutions is to provide a tall ward block with lifts grouped at the centre, and diagnostic, treatment, and outpatient departments arranged around its base in single-storey or two-storey buildings.

DENTAL HEALTH SERVICES

The total number of dental practitioners registered at the beginning of 1961 was 16,279, representing a net increase of

243 over 1959. The number of new British graduates—587—represented an increase of more than 20% over the average for the three previous years. The number of women dentists on the Register—1446—is about a thousand more than twenty-five years ago. "When more accommodation becomes available in dental teaching hospitals it is to be hoped that the number of woman students will be increased. Women dentists are particularly needed in the School Dental Service and as orthodontists. Unlike some other callings, dentistry is well adapted to part-time service, thus reducing the loss of trained women through marriage."

Infant-welfare Clinics

At the London School of Hygiene and Tropical Medicine students working for the D.P.H. are divided into small groups which investigate chosen subjects. The school's latest annual report [1] records the findings of a group which examined the infant and child health services of a London borough.

The group concluded that the services were still most necessary and on the whole were efficiently run. It deplored the lack of collaboration with general practitioners, but found that the responsibility for this defect was bilateral. Partly because 55% of mothers came to the clinic to have their babies weighed, and partly because the consultant pædiatrician regarded regular weighings as essential in "prediagnosis", the group concluded that routine weighing should be continued and that the weights should be charted graphically.

The group suggested that the quality of medical care could be improved by decreasing the number of medical officers and therefore making a substantial rise in salary possible. All such officers should work whole-time (the least satisfactory aspect of these services was the poor quality of the part-time medical staff). The health visitor already bore the brunt of the work and dealt with 75% of all attendances. This should be officially recognised, and on certain days she alone should run the clinic. The doctor would attend on special "doctor's days" when he could deal with difficult or referred cases, and see the 6% of mothers who, according to an opinion survey, preferred to see him. He should be obtainable on the telephone on the "health visitor's days", to cover the possibility of emergencies. Each local authority should appoint a consultant pædiatrician, competent in preventive pædiatrics, to keep the medical and health-visiting staff up-to-date with recent advances and to advise those administering the services.

1. London School of Hygiene and Tropical Medicine: report on the work of the school for the year 1960–61; p. 98.

Medicine and the Law

The Expert Witness

THE role of the expert witness, and the way in which his opinion is elicited by examination and cross-examination, have often been criticised; and a discussion on the subject was held recently at the Institute of Advanced Legal Studies (University of London) with Prof. Norman Anderson, the director, in the chair.

Dr. Lindesay Neustatter, senior psychiatrist, The Royal Northern Hospital, said he thought the present procedure worked satisfactorily provided the expert witness was objective in his opinions irrespective of the side which called him. That he would have this attitude must be made clear to counsel beforehand, however; and this (as Prof. Desmond Curran has pointed out) might mean that a search was made until someone was found who was prepared to say what was wanted. Dr. Neustatter agreed that the result might be that bad psychiatric evidence was given, but any harm that was done to psychiatry was, he thought, more than offset by the fact that the prisoner was given the fullest opportunity of presenting a point of view that he believed might help him, however unpopular and open to criticism it might be.

From his experience, at one time, of examining large numbers of juvenile delinquents for the courts, Dr. Neustatter felt it gradually became more difficult for someone in that position not to identify himself—albeit unconsciously—with authority. The Crown already had a prison doctor available, and if an official psychiatrist were appointed by the court his opinion would carry more weight than that of an independent psychiatrist called by the defence. The rights of the accused were too precious a heritage to be jeopardised, even at the risk of "bad" psychiatric evidence harming psychiatry. Dr. Neustatter admitted, however, that in murder trials the use of an independent psychiatrist available to either side had proved quite satisfactory.

In civil cases personal liberty was not at stake and a "neutral" psychiatrist might assist the court best. He favoured a panel of approved psychiatrists any one of whom could be called upon to give evidence, which would still leave much freedom of choice to plaintiff and defendant.

Dr. Neustatter pointed out that, if an expert was objective, his chances of embarrassment in the witness-box were very much less. Weak points in a case were far better conceded than dragged out under cross-examination. It was for counsel to right such matters—if they could be righted—in re-examination. Anyway, it was *not* for witnesses to make a case but to assist the court; nor did this necessarily harm the plaintiff. (In one case, where he had conceded that many of the plaintiff's statements were quite incredible, the latter nevertheless got £5000 damages.) One thing he did deplore was the barrister who put an expert in the box without troubling to discuss matters beforehand, and then asked questions which virtually forced the expert into being a hostile witness.

A barrister said the question was whether the expert should be called by the court, or whether the court should act as an umpire. It was for the court to clarify rather than enter the arena, and at present it was for the parties to decide what questions to raise and what witnesses to call. He pointed out that a coroner is in complete control of his court and can examine witnesses. In nullity cases, the court has an examiner who reports to it. In civil cases, the court can appoint an expert to report, and in probate cases the witnesses are called by the court and cross-examined by both sides. He felt that in general it would be better for the administration of justice that witnesses should be called by the court and not by the parties, for if they are called by the parties they identify themselves with one side or the other, and indeed counsel could not help but feel let down if he were not supported by "his" witness. If called by the court, witnesses could give an objective opinion with less feeling of tension.

During further discussion another barrister commented on the limitations of the jury system in civil cases, citing one case in which he, his opponent and the judge had all studied high-voltage currents intensively, and even then remained bewildered. It later turned out that two of the jury could neither read nor write.

A professor with knowledge of Continental practice

said he far preferred the English system. Thus in Germany, where the judge could examine a witness, there was the grave disadvantage that he knew the previous record of the accused.

Another speaker pointed out that, if two experts differ in a civil case, the appointment of a third expert by the court will not necessarily produce the right answer.

In conclusion, Dr. Richard Fox disagreed with Dr. Neustatter that a psychiatrist appointed by the court is in danger of becoming "prosecution minded", and on this appropriate note of doctors disagreeing the discussion closed.

The Employer and the Addict

From 1953 to 1957 a man was employed in a factory as foreman plater in the nickel-plating department, part of his duties being to supervise the degreasing of plated articles which were put through a tank containing trichlorethylene.

The tanks had to be cleaned out weekly. Regulations were displayed in the department forbidding operatives to enter the tank unless wearing breathing apparatus. The foreman had become addicted to the trichlorethylene vapour in 1949, when employed elsewhere, and on three occasions between 1953 and 1957 was found inside the tank without breathing apparatus and on other occasions held his head over the tank to inhale the vapour. The manager reprimanded him twice and the third time threatened him with dismissal if he did not discontinue the practice. Nevertheless he continued. On July 11, 1957, he died from a heart-attack resulting from trichlorethylene poisoning. On that day and the previous day he had spent some three hours in the degreasing plant.

The widow sued the employers for damages for the death of her husband and the employers alleged contributory negligence, in that the foreman had voluntarily and deliberately inhaled the vapours. The widow contended that if that were so the employers knew or ought to have known that continued exposure to the vapours could lead to a craving and were negligent in taking no steps to ensure that he did not inhale the vapours but continued to expose him to danger. Mr. Justice ELWES found that the manager knew that the foreman was addicted to the vapour, and that the employers had been negligent, and awarded the widow £5250 damages. The employers appealed.

Lord Justice SELLERS said that the question was whether there was any breach of duty by the employers in not doing more than they did when the facts came to their knowledge. Their duty was to take reasonable care for their employees' safety in all circumstances. In this case the work itself could be performed in perfect safety if the regulations were complied with. It was not the foreman's duty to clean out the tank, nor was it his task to put the articles through the liquid, but as foreman he had to see that that was done and to supervise it. If he had done that no harm would have come to him. But he had himself gone into the tank and had also held his head over the tank to inhale the fumes. There was no common law duty requiring an employer to go so far as to dismiss an employee because he thought that it was not for his good to do the work. He must do his best to make conditions as favourable as he could, but there was no obligation on him to deprive the employee of his employment and his livelihood because the employee was not prepared to take care of himself in circumstances where he could well have done so. The question of contributory negligence did not arise. His Lordship would allow the appeal.

Lord Justice UPJOHN, agreeing, said that the foreman, in deliberate disregard of the regulations and instructions, had deliberately exposed himself to risk against the employers' wishes. In those circumstances the employers could not be under any obligation or duty to him.

Lord Justice DIPLOCK agreed.

Jones *v.* Lionite Specialities, Ltd. *Court of Appeal:* Sellers, Upjohn, and Diplock, *L.JJ.* Nov. 30, 1961. Counsel and solicitors: Norman Richards, Q.C., and Norman Francis (T. S. Edwards & Son, Newport); Phillip Wien, Q.C., and Derek Howells (Barlow, Lyde & Gilbert for Cava & Lougher, Cardiff).

E. M. WELLWOOD
Barrister-at-Law.

Parliament

Props for the Elderly

ELDERLY people are a hardy lot these days, but many of them need some support, and, as Lord AMULREE pointed out in the House of Lords on Dec. 13, a true geriatric service is not merely hospital service. It should include the care of old people when they left hospital; the care of those who had no families or homes to go to; and the care of those who wanted some kind of assistance in their normal life. To begin with the hospital service, he suggested that we rather had the idea that the elderly sick were something special, whereas they were exactly the same as the younger sick, but they needed a better kind of treatment. Unfortunately, instead of better treatment, too often they got worse. They were looked after in beds in unsatisfactory old buildings where the medical, nursing, and ancillary staff was inadequate. When they were ready to come out of hospital there were too few places in convalescent or residential homes. If they were fit to lead a fairly independent life, there was lack of suitable housing and a shortage of domiciliary services to support them. Though we had some idea of the number of hospital beds needed for the elderly, we had never really known the number of beds needed for those who were unable to live in their own homes. If we could get some figures, we could then really begin to think about a proper coordinated service. In his view housing was the crux of the whole matter. If the population was well and appropriately housed, we should not need so much institutional and hospital accommodation, and people would be much more comfortable and happier.

Baroness SWANBOROUGH wanted to see domiciliary care made as perfect as possible. For this a subtle and intelligent integration of voluntary services and statutory aid was necessary. People living in their own homes did not need constant care and attention but only props. Cooperation was needed between the hospital and local-authority services. She would rather that old people had slightly less scientific care given to them, and that they could live their own life in their own way. For those who could not remain in their own homes, she would like to see provided places more on the basis of attention and care in a practical way, rather than imitation hospitals.

Baroness SUMMERSKILL agreed that the great need was for suitable accommodation for the elderly who were anxious to remain independent but who needed some supervision. Their chief needs were privacy and warmth. But she would like to see some system of notification started to ensure that they had adequate support. The lonely often needed individual help, and to compel them to attend a club was only to add to their misery. She regretted that there were not more home helps. But society shortsightedly underpaid these invaluable women.

The average cost of a hospital bed was £40 a week, yet the authorities haggled over 6*d*. an hour for a home help.

Lord STONHAM thought that the " great and glaring need " was for one authority or person in each area to be responsible for the old people, and he believed that person should be the medical officer of health. He agreed that a register of old people was essential. Different services were provided by different authorities, and he asked the Government to consider bringing in a measure which would end the uncertainty as to whether certain services could be provided or not, who was allowed to pay for them, and whether or not they would qualify for a grant. A wider range of powers and complete statutory overall responsibility should be given to the medical officer of health.

The BISHOP OF SHEFFIELD suggested that as it was important not to associate the care of the aged too much with ill health, it should be put under the social-care department rather than the medical officer of health.

Lord NEWTON, replying for the Government, reviewed the expansion of the geriatric services in recent years. The number of nurses employed whole and part time in the home nursing service had risen from 7000 in 1948 to 10,000 in 1960 and the number of home helps from 11,000 to 49,000. During 1960 the elderly and chronic sick represented about 75% of all those helped. In 1953 the figure was only about 58%. Authorities had been given every encouragement to provide suitable housing for the elderly. A special subsidy of £10 a year for sixty years for one-bedroom dwellings was retained when the general needs subsidy was abolished in 1956. Under the new subsidy arrangements now in force, local authorities which satisfied the test of financial need in the Housing Act, 1961, would get a much bigger subsidy—£24 for any kind of house, flat, or flatlet which they built for old people. The number of people in residential accommodation had increased from 46,468 in 1949 to 84,556 in 1960. Since the war 1184 modern small homes had been opened. Nevertheless local authorities had failed to overtake the demand. There was still a shortage of accommodation for the very infirm, though the amount of capital investment authorised in England and Wales for the provision of old people's homes had risen from about £2½ million in 1956–57 to £8¼ million in 1960–61. The number of staffed geriatric and chronic sick beds rose from 57,713 in 1957 to 59,183 in 1960, and more than 100 geriatric departments under specialist physicians were now provided by hospital authorities in England and Wales. With the coming growth in the proportion of elderly people, it would be necessary to continue making adequate provision to meet the demand for long-stay hospital accommodation. But apart from some local shortages, generally speaking the need at present was not for more beds but rather for better facilities and supporting services.

Family Allowances and National Insurance Bill

THIS Bill, which was introduced in the Commons on Nov. 1, was read a third time on Dec. 13. If, as is hoped, it goes through all its stages in the House of Lords and receives the Royal Assent before the House adjourns for Christmas, the supplementary allowances which it provides for the totally disabled older industrial casualty will start in the third week in January. Allowances for the partially disabled, which will require an affirmative resolution, will be paid with as little delay as possible. The provision of family allowances for incapacitated children up to the age of 16 will come into force as soon as the Bill is passed.

QUESTION TIME

Doctors from Overseas

Mr. R. W. SORENSEN asked the Minister of Health approximately how many of the present number of local practitioners and of hospital medical staff, respectively, were of overseas Commonwealth, British colonial, Irish, and foreign origin; how many of these were trained in this country; and how many doctors of British birth had left for overseas service during the past five years. Mr. ENOCH POWELL replied: In March last, 2505 of the junior medical staff in hospitals in Great Britain were from the Colonies and other Commonwealth countries, 375 from the Irish Republic, and 748 from other countries overseas. 392 of the total had qualified in Great Britain. Information is not available for other hospital staff or general practitioners, nor are the figures requested in the last part of the question.

Merit Awards for General Practitioners?

Mr. L. A. PAVITT asked the Minister if any decision had yet been reached as to the disposal of £500,000 which, in accepting the recommendations of the Pilkington Committee, the Government agreed to devote to rewarding merit among general practitioners in the National Health Service.—Mr. POWELL replied: No, Sir. The joint working party on this has met once. A further meeting was postponed at the request of the profession.

Welfare Foods

Mr. KENNETH ROBINSON asked the Minister if, in view of official figures showing a drop of more than 70% in the take-up of orange juice and cod-liver oil at the new prices, he would reconsider his policy regarding subsidy on vitamin supplements and now introduce regulations to amend the Welfare Foods (Amendment) Orders, 1961. Mr. POWELL: No, Sir. Welfare foods remain a cheap source of vitamins along with the many other sources now available. They can be supplied free if necessary. Their availability has been further publicised. Mrs. SLATER: Does not the Minister realise that local-authority social workers are deeply concerned about this 70% drop in the intake of welfare foods? These are the people who visit the homes and who know the people who need them most are those who are failing to get them? Mr. POWELL: I am sure that the vast majority of parents and families are able and willing to pay for this supplement for their families. The problem concerns a very small minority and a general subsidy is not justified.

Radiographers

Mrs. SLATER asked the Minister if he would state the cost to the National Health Service of the employment of radiographers supplied by a private company as against their direct employment by his department. Miss EDITH PITT: I regret no useful estimate can be made. Mrs. SLATER: Is it reasonable to allow a private company to supply radiographers to the National Health Service at a higher salary than the Ministry is prepared to pay them? Miss PITT: It may be reasonable as a temporary measure when there is pressure or need. Mr. KENNETH ROBINSON: Would not the Minister agree that it is wrong that a hospital authority should be forced to go outside the National Health Service to a private company to get radiographers? Does not the Minister appreciate that unless the Ministry does something about this, it will lead to a vicious circle because more and more radiographers will leave the hospital service and go to private firms which can pay them more? Miss PITT: It is essential to the health service that a radiographer should be available, and while there is a shortage, I see no reason why hospitals should not employ an outside service, provided that it is a temporary measure.

Leprosy

Mrs. BARBARA CASTLE asked the Minister how many infectious cases of leprosy there were in Manchester on Oct. 31, 1961, and Oct. 31, 1951, respectively; and how many of these cases occurred among the West Indian population.—Miss PITT replied: 1 and 0 respectively. Notifications do not give country of origin. Mrs. CASTLE: Do not those figures, showing that there is only 1 case of infectious leprosy in Manchester and providing no evidence that the infection is coming from among the West Indian immigrant population, once again prove that attempts have been made to support the Commonwealth Immigrants Bill on grounds which are quite unfounded and that public opinion was aroused during the Moss Side by-election in favour of the Government measure with no justification whatever? Mr. SPEAKER: That supplementary question does not appear to relate to a matter within the responsibility of the Minister of Health.

In England Now

A Running Commentary by Peripatetic Correspondents

I WAS rather surprised last week when my wife suddenly asked if I would like an electric razor for my birthday, for I already have one. What she had said, though, was electric *eraser*, a thing I didn't know existed. But it turned out to be a fascinating little shiny chrome cylinder, about 6 in. long, rather like a miniature space-rocket. The base holds the fuel, a small dry battery; the upper part the mechanism, the apex tapering to the " space-capsule " at the tip. This is the eraser, which holds a rubber or a brush of steel—or glass-hair according to the type of paper surface to be abraded. When a button is pressed, the eraser revolves at high speed and, held lightly against the paper, removes ink- or type-written lettering in masterly fashion. I can imagine the cosmetic surgeon swiping the gadget from his secretary to apply it to his patients' skin blemishes.

Of course, since I've had my new toy my typing has become tiresomely accurate. But I would appreciate it if the boffins could now dream up an electric *transposer*, something that would straighten out accidnets and clarify what I've siad.

* * *

News from School.—" The essay paper was quite easy. I wrote on ' Myself ', although it sounds egositic it was the easiest of all the essays. I am enclosing information on the Committee of 100 . . . they have asked me to help them in the holidays. I hope you don't mind if I do this, but I now feel very strongly about the subject, and everyone should be made to understand the importance of disarmament. I bought Russells book, and its interesting and frightening . . . I seem to be trying to press-gang the family to conform. Anyway I have had success here. I took the information to Mr. X and Mr. Y. I doubt whether Mr. X looked at it, but Mr. Y has altered his views. Also Jimme T. is confirted, having read Russell's book."

* * *

Eponyms have their hazards. We were discussing the case of a patient who had been under the care of my surgical colleagues. " What was done ? " I asked. " They don't seem to have been very satisfied with the operation," said my house-physician. " Why not ? " " Well, the notes start off by describing it as a modified battle."

* * *

Have you seen these new protective felt hats for women motorists ? . . . You wouldn't be seen dead in one ? Exactly, that's why I bought mine.

* * *

" Stringency is the mother of efficiency "—The Minister of Health.

Let's be stringent and efficient,
Be phlegmatic and concise,
Give at all times just sufficient,
Be judicious, don't be nice.

Let's be strict and ever Willink
To take cuts of ten per cent;
Let us save an honest shillink
Clipping down medicament.

Let us study economics
In our efforts to be smart,
Put more water in the tonics
And the horse behind the cart.

Let us export all clinicians
Who reject our rule of thumb.
There are overseas physicians—
If we beckon, they will come.

Well goodness gracious me,
Is this Powell's policy ?

Letters to the Editor

RECONSTRUCTING THE N.H.S. ?

SIR,—Many are now looking back and appraising the National Health Service since 1948. Was it soundly built or has it basic flaws ? Has it evolved as we hoped ? Does it lack the power of spontaneous advance ? Could some of its apparent success be due to the momentum of services as they were before 1948 ? Is there a risk, as this lessens, of the tempo slowing, of expensive circus or even Brownian movement being mistaken for progress and of smug Northcote Parkinsonism, with its bad prognosis, supervening ?

This is why the observations of Dr. Seale,[1] Dr. Lees,[2] and Dr. Fox [3] have aroused much interest.

I agree with Dr. Seale about the inefficiency of management and the detrimental effect of this and of the absence of a well-defined objective upon medical efficiency. What should the objective of a personal health service be ? How increasingly woolly and diffuse it is possible to be over this! We need a simple clarity about it. The sole objective should be to bring, without a moment's delay, the highest standard of medical treatment that can be attained in the present state of knowledge to every person who stands in need. Just this and nothing more. All else is secondary.

Let there be a continual sense of urgency and let us feel ashamed when any one person suffers or dies today for lack of this highest standard. We should forget about the " best medical care the nation can afford", to use Dr. Seale's phrase, which I do not like. If the objective is to be the best we can afford then our eye will always be upon what we can afford and not upon what we need. We do not let sailors drown off our coasts because we have only the best lifeboats we can afford. We have the lifeboats we need and we do not waste money on secondary matters such as over-elaborate lifeboat sheds or too many administrative offices.

Dr. Lees is right when he says that there is an " inadequacy of forces making for improvement " and that such forces are not built into the service. He over-emphasises, however, the amount of personal ill health that can be treated as a marketable commodity and is too pessimistic about attaining efficiency in a public service. All major illness should be handled like a catastrophe. Dr. Lees does not see the difference between the fire service instantly available for a burning home and the free choice of the owner whether or not to spend his money on fire-insurance premiums. Talking of fire, it's plenty in its belly the service wants.

Dr. Fox's conception of the " family " or " personal " doctor has much that is attractive, but the simple over-riding objective remains. The family doctor should primarily be as skilled in medicine as it is possible to make him. The family or personal side should come afterwards. A wonderfully sympathetic and reassuring doctor, who treats the whole patient, is not much good if he treats that part of him which suffers from an early coronary thrombosis as indigestion or rheumatism—something which is not exactly unknown. Many of us have a family garage man but he's not much good if he cannot cure a choked jet, even if he does look after the " whole car ".

The service needs a central body inside it, as Sir George Schuster rightly advises, like the general staff of the Armed Forces, publicly constituted and charged to attain

1. *Lancet*, 1961, ii, 476, 977.
2. Health Through Choice. London, 1961.
3. *Lancet*, 1960, i, 743.

the one overriding objective I have defined. Perhaps one day we shall have a Haldane to set it up. The same sort of thing seems to be coming now in the new long-term planning machine that is being established to manage the nation's economy.

Meanwhile, the spur, and a sharp one at that, needs constant application.

London, W.1. T. ROWLAND HILL.

LONDON'S GOVERNMENT

SIR,—Your endorsement (Dec. 9) of the Government's proposals for the reorganisation of London's local government says much for your democratic ideals, but little for your administrative practicality.

Clearly some radical change in local government methods must come when the population governed and the responsibilities to be shouldered have altered as much as they have done during the last half century in the area (and beyond) covered by the Herbert Commission. But what are we to make of a plan which, in the name of strengthening democratic principles, preserves unaltered from medieval times the power and privileges of the City of London? What is the precise democratic accountability of a body as remote from the elector as the proposed new Greater London Council? And how is a new London borough (*minimum* population 200,000) significantly closer to the electorate than an existing L.C.C. division (*average* population 350,000)?

To oppose these changes merely because it is easier and more comfortable to stay as we are would merit your condemnation. But to oppose them on the ground that they carry us away from a desirable objective instead of towards it, should, I submit, command at least your sympathetic consideration.

Your remarks were concerned with the general programme outlined in the Government's white-paper. Had you been concerned with the detailed consideration of the meaning of those proposals for the health services in London, your endorsement must surely have been more qualified.

The present tripartite structure of the National Health Service is increasingly recognised as a barrier to the effective organisation and integration of those services. The wide area unified under the London County Council and the effectiveness of its organisation has allowed it to make some preliminary moves to overcome the schisms. In recent months, it has called together representatives of all the administrative bodies of the National Health Service—the four metropolitan regional hospital boards, the teaching hospitals, the London Local Medical Committee, and the London Executive Council—to discuss two of the topics which are leading to the end of tripartitism: the implementation of the Mental Health Act, and the organisation of the ambulance services. (Others, I hope, will follow: the state of the maternity services, despite Cranbrook, clearly calls for this sort of discussion.)

If the changes proposed actually occur, what body can continue this important exploratory work? Faced with the uneven distribution of hospitals (and particularly of maternity hospitals) in the Greater London area, the new boroughs with their responsibility for the personal health services will be too small and fragmented to take an effective lead in the matter—though they will be under even greater pressure than the more comprehensive L.C.C. to do so if they are to assist their inhabitants.

The difficulties which will face the new boroughs in organising the associated services, especially the welfare and children's services, expose the enormity of the cost compared with the dubious gain. Children's homes and welfare homes are not numerically nor topographically allocated one to each new borough—and even less so are the trained staff. Ad-hoc joint action by a group of new boroughs is the only solution I can see.

Even more tragic will be the fate of the marvellous—but unpublicised—work done by the L.C.C. for the deaf child, for the maladjusted child, for sheltering the handicapped adult, to name only a few services which could be provided only by an authority like the L.C.C. covering a large population (i.e., over 1 million).

As an elected member of the L.C.C., I must be expected to spring to its defence. I do so without blinkers on. I would welcome any alteration in local government organisation and responsibility which was sufficiently bold and contemporaneous to recognise the need for regionalisation, not fragmentation; and which proposed changes that allowed the elector (who has much speedier and more effective communication with his elected representative than his grandfather had) to exercise his influence over a wider range of integrated services than he will be permitted to do if the Government pursues its present proposals.

London, S.W.17. DAVID KERR.

PRISON DOCTORS

SIR,—The mental picture irresistibly brought to mind by Dr. Kidd's suggestion (Nov. 25) that " local mental hospitals could spare a consultant for one afternoon a week, not necessarily to treat prisoners but to advise the Governor and Prison Medical Officer, who, indeed, through no fault of his own is not aware of the newer and more effective methods of diminishing violence " is one not unlike that of some medical St. Augustine bestowing the blessings of the gospel on those of the bemused and wondering heathen who come to hear him.

We should be interested to learn whether Dr. Kidd has ever had the opportunity of visiting a prison hospital or of discussing one of the more obvious problems of the prison medical officer—namely, the treatment of the aggressive psychopathic offender in the overcrowded local prison. What is certain, from his remarks, is that he is wholly ignorant of the yearly expenditure on the various anti-depressant and tranquillising drugs that are prescribed by these doctors and therefore included as an item in the annual account sheet.

Our own view—a personal one—about the use of straitjackets in prison is that it may well be possible, and it would certainly be desirable, to abolish them, although that is not to say that some modified form of restraint may have to be retained for use as a temporary measure with violent patients until the drug of choice has begun to take effect. We are referring here not to the schizophrenics and manic-depressives but chiefly to those psychopathic and hysterical offenders who, emotionally abnormal though they undoubtedly are, can rarely be said to be suffering from any form of psychotic illness. These constitute a large group of patients whom the psychiatric hospitals, very understandably, both abhor and are most reluctant to accept; and, should they succeed in slipping through the restraining net into the hospital, they are rarely discouraged from discharging themselves (always supposing they have failed to abscond after a brief stay there); and they are then often placed on the hospital's " black-list " to ensure that they do not return. Yet these are the very individuals who have to be retained on the

prison roll not merely for months but, frequently, for years at a time.

To describe not only the strait-jacket but also the padded-room as " instruments of torture ", as Dr. Kidd does, is merely to make meaningless certain words in common use in the English language, and it is as well, Sir, that your readers should be made aware of this. To state that no British mental hospital known to Dr. Kidd, " however backward ", has made use of the padded-room during the last quarter-century merely serves to prove that Dr. Kidd's knowledge of these hospitals is considerably more limited than he can suppose. It may surprise him to be told that certain progressive hospitals possessed and used such rooms up to a very few years ago. They have now been replaced by stripped and shuttered side-rooms, and it is a debatable point whether padded-rooms, shorn of those intense emotional overtones which surround them, and which seem to have so affected Dr. Kidd, are in any way more unpleasant than these side-rooms. So long as prison hospitals must cater for overcrowded prisons (and this means a lack of side-rooms with adequately protected or strategically placed windows), so long will the padded-room be required if self-inflicted injuries and the putting out of commission of much-needed hospital accommodation are to be prevented. Although the padded-room is essential, only comparatively rarely, in our experience, has it to be brought into use.

That every doctor in the Prison Service would welcome the opportunity to spend a period of study in a psychiatric hospital is probably true. We are not attempting to be patronising, however, when we suggest that, among those consultant psychiatrists who find themselves engaged, however occasionally, in the forensic branch of this specialty, not a few, in our opinion, would equally benefit from a similar period among the varied and unusual psychiatric material to be found in the prisons.

I. A. ANGUS
H.M. Prison, Winchester. J. K. LOTINGA.

NIGHT-CALL SERVICES

SIR,—I have followed this correspondence with interest because in the course of my duties as regional medical admissions officer I have not infrequently had cause to lament the advent of the deputy night-call service.

When the scheme started I determined to await developments with an open mind. To begin with I even welcomed the influx of cases from what were obviously part-time postgraduate registrars: here was an opportunity for the specialist of tomorrow to learn at first hand something of the problems of general practice.

Since those early days the postgraduate deputy has been much less in evidence. With an ever-increasing case-load, particularly during the worst months of the winter, considerations of expediency seem to have crept into the service.

When efforts to find a bed have failed I generally try to assess the urgency of a case by direct discussion with the doctor concerned. Usually a general practitioner can be contacted during the night without undue delay. In the case of a deputy service, when the doctor may have several patients to visit, anything up to two hours may elapse before contact is established. In theory, one is supposed to be able to locate a doctor via the service control and short-wave radio: in practice the " chap " I want to contact nearly always seems to be using a car without radio, or if it has one, he seems unable to answer it.

On one occasion when I did manage to speak to the doctor I was told quite frankly " Oh well, we have been told to get everything into hospital ". On another occasion, when discussing the possibility of morphine at home, I got the reply " I quite agree, doctor, but you see we are not allowed to use morphine ". (This seems to corroborate " Diplomate " 's comment of Dec. 2.)

These considerations apart, one cannot help feeling that under the night-call scheme the complete stranger called to an apparent emergency is more likely to decide on hospital admission than the patient's family practitioner or partner to whom Mrs. X's " abdominal emergency " has always been assessed at its true value. Then again, an appeal to a local practitioner in times of acute bed shortage is likely to be considered with due regard to this shortage of beds and staff, and in loyalty to that situation—considerations that scarcely apply to the peripatetic, here today, gone tomorrow locum.

I appreciate that to the single-handed practitioner a deputy is essential, but to my mind a rota of four or five is more realistic.

Metropolitan Regional Hospital Boards,
Regional Admissions Office,
London, S.E.1. A. L. DE SILVA.

PLASTIC SURGEONS AND THE G.M.C.

SIR,—The British Association of Plastic Surgeons has taken notice of the observations of the President of the Disciplinary Committee of the General Medical Council on Nov. 25 with the respect which is due to the authoritative tribunal of all registered British medical practitioners.

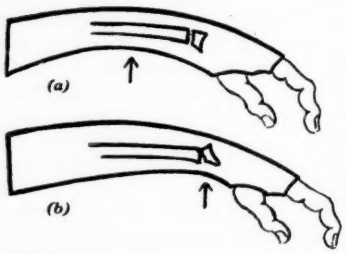
Our colleagues of the medical profession may be assured that the serious misgivings will be allayed.

GEORGE H. MORLEY
President, British Association of Plastic Surgeons.
London, W.C.2.

COLLES' FRACTURE

SIR,—I must congratulate Mr. Stone (Dec. 9) on his efforts to prevent the so-called inevitable late deformity in Colles' fractures in the elderly. Like him, I repudiate the oft-repeated notion that collapse of the reduction cannot be prevented, but I have tried to solve the problem at an earlier stage in treatment. Instead of waiting two weeks for the relapse and then repeating the reduction, I change the plaster after three or four days and again, if necessary, after one week. I have found that the repeated application of moulded plasters with the forearm fully pronated gives a very high proportion of good results. My failures I attribute to lack of vigilance in carrying out this regimen.

The key to success in holding Colles' fractures is correct moulding of the plaster. Many textbooks show faulty plasters which will inevitably lead to relapse. The secret is to mould the plaster with a concavity opposite the upper fragment —that is, well up the forearm (fig. *a*). If the plaster is moulded over the front of the lower fragment, as is commonly illustrated, then the plaster will actively encourage relapse of deformity (fig. *b*).



Another common fallacy is the notion that a poor reduction of a Colles' fracture in the elderly usually gives good results. This may be merely a reflection on the tolerance and patience of the elderly. A pertinent question is, how long does it take to achieve this result? I agree with Mr. Stone that the elderly female living alone needs the best reduction possible, because good reductions always lead to rapid recovery of function without the need for physiotherapy.

How much valuable time is being spent in physiotherapy departments up and down the country, waxing,

massaging, and exercising deformed wrists back to function? It is surely more profitable to spend a small fraction of this time in the prevention of relapse which occurs in the early stages of treatment.

Bridgend General Hospital. A. W. FOWLER.

HALOTHANE

SIR,—Dr. Langton Hewer (Nov. 25) wisely draws attention to the possibility of carbon-dioxide build-up when halothane is used in the closed circuit. I have always been aware of this danger and drew attention to it in my article[1] on the use of the Goldman halothane vaporiser in the closed circuit.

I elaborated this point in a paper at the Toronto conference in October, 1960. I showed that measurable carbon-dioxide retention does occur in the closed circuit even with a slight "blow-off" when the minute volume falls below 3 litres per minute. Incidentally there is evidence that even when breathing high oxygen concentrations there is the possibility of hæmoglobin oxygen desaturation at this minute volume.

I therefore advise the use of a volumeter in the circuit. If the minute volume falls below this critical level it is better to reduce the halothane concentration and aid the respiration.

Worthing Hospital,
Worthing, Sussex. F. R. GUSTERSON.

TRACHEOTOMY AND EMPHYSEMA

SIR,—While one must congratulate Dr. Harris and Dr. Houston (Nov. 25) on their efforts in the treatment of acute chest infection in emphysema, I am a little disturbed by their enthusiasm for tracheotomy. I think, and I am sure that others who have had experience of this problem will agree, that efforts should be directed, not to more and earlier tracheotomies, but to the establishment of an emphysema clinic where the bad-risk cases (as determined by the level of pCO_2) can be sought out.

It is important that these bad-risk cases should be admitted to hospital early, and this can be achieved by close cooperation between consultants, G.P.s, and patients' relatives. When this is done, my experience has been that the number of tracheotomies falls rather than increases. This has helped a lot, since experience has taught me that correct, and I repeat correct, management of patients with tracheotomies who need assisted respiration cannot be delegated to juniors, let alone the nursing staff, and the burden on the consultant becomes an insupportable one. I have found that the use of an analeptic, physiotherapy, and assisted respiration alone are often successful in bad-risk patients, if they are admitted early in their relapse.

It is well known that bad-risk cases, such as Dr. Harris and Dr. Houston describe, who have been treated by tracheotomies may relapse more than once. It would be interesting to know how many of their patients refuse tracheotomy a second time; more than one of mine has, but fortunately the treatment which I have outlined above has been successful.

Chase Farm Hospital,
Enfield,
Middlesex. T. SIMPSON.

SIR,—Dr. Harris and Dr. Houston say that "the striking feature in each of our cases was the patient's inability to cough up his thick tenacious secretions". Dr. Broom (Dec. 9) suggests that it might be worth considering the possibility of therapeutic bronchial lavage in order to overcome the problem of sticky suffocating secretions.

Although tracheotomy and/or bronchial lavage are at times necessary, they are obviously not ideal in seriously ill patients. They would have to be used less often if an effective mucolytic agent were available, but so far all such substances other than steam have proved disappointing. Recently at this hospital a new oral drug, 'Organidin' (Denver), has been under preliminary investigation. This substance, which contains organically bound iodine, was first used in America in the treatment of thyroid disease. It was found to be of limited value in such conditions, but by chance some of the patients who, in addition, had thick tenacious sputum found that this was much easier to expectorate. In our pilot study we have also found that it does appear very helpful in loosening viscid sputum both in bronchitis and in acute asthma. In view of this favourable clinical impression we are now seeking confirmation of its efficacy by a double-blind trial under the control of a statistician.

We draw attention to this drug in case others may also like to submit it to controlled investigations.

Ashford Hospital,
Ashford, Middlesex. P. E. BALDRY.

1. *Brit. J. Anæsth.* 1960, **32**, 388.

INDUSTRY AND PROFESSION

SIR,—Twice[1 2] in recent months you have drawn attention to the difficulties that can arise between the pharmaceutical industry and the medical profession, with special reference to the organisation of clinical trials. Two correspondents[3 4] have mentioned general-practitioner organisations for undertaking such trials. The activities and methods of the clinical trials subcommittee of the research committee of the British Tuberculosis Association are also relevant.

Under the chairmanship of Dr. R. W. Riddell and at first under the title of the Clinical Trials Organisation, this subcommittee was formed in 1957 to advise pharmaceutical firms on the use of new drugs or combinations of drugs in tuberculosis and diseases of the chest, and to arrange clinical trials of those materials considered to warrant further investigation. A registration fee, originally 10 guineas, now 25 guineas, is charged; if a clinical trial is carried out all expenses are borne by the firms concerned. This is achieved by the firm making a grant to the B.T.A. who are then responsible for payments for technicians and secretarial and other help. All financial matters are dealt with through the offices of the Association; there are no financial dealings directly between the firm and the individuals doing the work. These arrangements appear to meet all the important requirements laid down in point (4) of your correspondent's article in October.[5]

Reports on results of clinical trials are prepared by the individuals carrying out or coordinating the work on behalf of the clinical trials subcommittee. Preliminary reports are discussed with the medical advisers of the firm: final reports are sent to the firm and to the research committee. It is a fundamental requirement that the decision on publication lies solely with the research committee and not with the firm. Firms are, of course, free to quote in their advertisements the published results of trials, but prior approval must be obtained if the research committee of the B.T.A. is mentioned in the advertisement other than in the list of references to published work.

22 requests from firms for clinical trials or advice have been considered by the subcommittee; 6 cooperative trials have been arranged (2 completed, 4 in progress). In 4 cases laboratory investigations have been sponsored. In 12, no clinical or laboratory study has been undertaken, but the firm has been given the advice of the committee on the evidence submitted to it concerning their product.

The research committee of the B.T.A. believe that the work of the clinical trials subcommittee meets a real need on both sides. The pharmaceutical firm is given an opportunity of submitting new materials to clinical trial with proper and independent safeguards. Physicans can cooperate in these trials without either being out-of-pocket in meeting the proper expenses of the trial, or feeling under any sort of obligation to

1. *Lancet*, Aug. 19, 1961, pp. 411, 421.
2. *ibid.* Oct. 14, 1961, p. 869.
3. *ibid.* Oct. 7, 1961, p. 827.
4. *ibid.* Oct. 28, 1961, p. 985.
5. *ibid.* p. 891.

the firm. In this way truly independent assessments of new materials that have undergone sufficient preliminary investigation can fairly be undertaken. One other important service performed is that individual members of the B.T.A. can receive on request a list of materials referred by firms to the clinical trials subcommittee [6]: this can be of considerable value in assessing the position of a new product brought to a member's attention by a pharmaceutical firm.

V. H. SPRINGETT
Chairman, Research Committee,
British Tuberculosis Association.
Birmingham.

IS NOTIFICATION USEFUL?

SIR,—It occurred to me that your comment of Dec. 9 (p. 1314) gave a not quite accurate picture of the order in which diseases were made notifiable.

Meningococcal meningitis and poliomyelitis were, for example, made notifiable in the first instance by the Public Health (Cerebro-spinal Fever and Acute Poliomyelitis) Regulations, 1912, and acute encephalitis lethargica and acute polio-encephalitis by the Public Health (Acute Encephalitis Lethargica and Acute Polio-encephalitis) Regulations, 1918 and 1919. Thus, when in 1949 "some infectious diseases of the nervous system" were added to your list, it was by way of a modifying and consolidating regulation, just as, for instance, earlier rules relating to the notification of tuberculosis were consolidated in the Public Health (Tuberculosis) Regulations, 1930.

As a practising medical officer of health, I believe that the notification of infectious diseases still fulfils a useful purpose, though how far it does so is determined by the speed and thoroughness with which general practitioners carry it out. In my area there is a very great discrepancy among the practitioners in respect of the notification of suspected food-poisoning. My council has recently obtained the consent of the Ministry of Health to make infective hepatitis locally notifiable for an experimental three years from Oct. 1, 1960, and I am already surprised by the many cases notified to date, though I cannot claim that any significant pattern has so far revealed itself.

JOSEPH V. WALKER
Medical Officer of Health.
Health Department,
Darlington.

CHILD PSYCHIATRY

SIR,—It is unfortunate that the memorandum from the Association of Undergraduate Teachers of Psychiatry (which appeared in your issue of Dec. 2) should give a somewhat misleading picture of present-day child-guidance practice in this country.

Surely the majority of child-guidance clinics sponsored jointly by regional hospital board and local authority, or by local authority alone, function as part of the school health services, and are therefore linked with the "medical" rather than the purely "educational" services of the authority. And the very great majority of such child-guidance clinics do have a psychiatrist as clinic director.

It may well be true that most of the children seen at teaching-hospital clinics will be seriously disturbed emotionally. After all, it is a general complaint in most fields of medicine that the teaching hospital tends to concentrate too much on the rarer disabilities at the expense of those "simpler" conditions which are much more commonly seen in non-teaching-hospital practice. Certainly most other child-guidance or child-psychiatry clinics will see an appreciable proportion of children who are not seriously disturbed, but who are reacting to immediate stresses in their environment. Such children, and their parents (to say nothing of the teacher, the hostel staff, the child-care officer, or the many other possible vital figures in the child's environment) may need help in the form of case-work, simple psychotherapy, support, or advice according to their individual needs. "Guidance" would seem to be a reasonable omnibus word to cover all these various forms of help.

The diagnostic assessment as to which children need "guidance", and which need more intensive therapy, as well as the provision of either type of help, needs the professional skills of the whole child-guidance team, and not least of all the child psychiatrist. If the psychiatrist were to withdraw from his role in the child-guidance clinic, a very serious gap would be left in the provision of adequate help for the child in need of such assistance.

T. A. RATCLIFFE.

PREVENTION OF FŒTAL ABNORMALITIES

SIR,—The title of Dr. Cross's paper (Nov. 18) implies that in his series of 16 women with normal babies there would have been a number of anencephalic or otherwise malformed offspring, which his regimen rendered normal. The probability of recurrence of anencephaly (or spina bifida aperta) in the sibs of babies with anencephaly is about 3%. There is little information on the risk after two anencephalic babies, but it is probably not more than about 10%. Without any effect of treatment, therefore, one would have expected about 1 anencephalic baby. Surely the fact that there were none does not justify claiming a beneficial effect of treatment?

F. CLARKE FRASER.
Department of Genetics,
McGill University,
Montreal.

SIR,—It is widely appreciated that valueless therapies may be temporarily accepted because initial reports were, in retrospect, incomplete or misleading. It is equally, but less obviously, true that valuable therapies may be rejected because initial reports appear unconvincing.

The title of Dr. Cross's paper (Nov. 18) implies that the regimen he describes is capable of preventing fœtal abnormalities, but he adduces no evidence to support this. The multitude of his recommendations, including injection of bismuth and the forbidding of hair-washing during pregnancy, must inevitably cause some scepticism, but it would be tragic if something of true value was overlooked, like the foxglove in the witch's potion.

Dr. Cross owes it to himself and to us to explain how his cases were selected, whether he has other treated cases not included, whether he had any control cases, and what was the outcome of the unsuccessful pregnancies not detailed in his table. (In the heading of the last column of the table, "frequency" should read "pregnancy".) Although a woman who has given birth to two anencephalics has a much increased risk of doing so again, the probability is that her next baby will be normal. Dr. Cross must convince us that he has strengthened this probability.

R. W. SMITHELLS.
Alder Hey Children's Hospital,
Liverpool.

**We showed Dr. Smithells' letter to Dr. Cross whose reply follows—ED. L.

SIR,—In the infertility department of the Rotunda Hospital the treatment of different types of secondary sterility is as follows:

(1) *Multiple miscarriage, habitual abortion, and multiple death of fœtus.* (*a*) *If the patient is not pregnant when first seen.* Her marriage is investigated and her pelvis examined, keeping a sharp look out for torn cervix and genital malformation. Torn cervix is dealt with by Falkiner trachelorrhapy and any other condition needing surgery is treated. The patient and her husband are then put on a pro-fertility preconceptional regimen. (*b*) *If the patient is pregnant when first seen.* She is given vitamins and bismuth throughout pregnancy, as detailed in my article.

(2) *Repeated fœtal abnormalities, anencephaly, and spina*

6. *Tubercle, Lond.* 1961, **42**, 373.

bifida. (*a*) *If the patient is not pregnant when first seen.* A pre-conceptional pro-fertility regimen is instituted in both the husband and the wife, and when she becomes pregnant she is put on a special routine including vitamins and bismuth. (*b*) *If she is already pregnant when first seen* the special routine is applied.

In group 1(*b*), where the patient was pregnant when first seen, in 99 treated cases live babies were born in 81, abortion or miscarriage followed in 10, the baby was born dead in 6, and died in the neonatal period in 2.

In group 2(*a*), of 16 women, 15 have given birth to healthy infants and the 16th remains well and is near term.

The treatment may be criticised as blunderbuss, but it seems to me to be effective.

Rotunda Hospital, Dublin. RAYMOND C. CROSS.

CERVICAL FACTOR IN FERTILITY CONTROL

SIR,—In his article of Nov. 18 on fertility and health in India, Dr. Maung Stein discussed some difficulties arising from the use of gonadotrophins for inhibition of ovulation and he emphasised the importance of the cervical factor for fertility control. He writes, " What we need, therefore, is a compound which, when taken by mouth, will thicken cervical mucus without inhibition of ovulation ".

There are very likely other ways of making the cervical mucus impermeable to sperms. It was shown with labelled compounds that sodium iodide and potassium thiocyanate, given intravenously, appear in the cervical mucus within minutes and are rapidly concentrated there. Even a large molecule like albumin appears quickly in the cervical mucus.[1] Since these studies demonstrated that the human uterine cervix has a definite excretory function, it might be profitable to search for non-toxic and non-hormonal drugs which are excreted with and concentrated in the cervical mucus and which would have any of these three actions: lower the pH below 5, thicken the mucus, make the mucus spermatotoxic. Any one of these effects would rather quickly produce a mucus impermeable to sperms. Furthermore, it might be possible to treat cervicitis from " within " by taking advantage of the excretion of an appropriate drug with the cervical mucus and concentration of the drug within it.

The excretory function of the human uterine cervix deserves intensive investigation because of its possible importance in fertility control.

Department of Medicine,
University of Colorado School of Medicine,
Denver, Colorado. K. N. VON KAULLA.

DELIBERATE EMERGENCIES

SIR,—Mr. Garland (Dec. 9) says that " A woman should no more have her baby at home than she should have a dilatation and curettage on the kitchen table ". The more I hear of practice in London with its difficulties of transport, ambulances, differences between general practitioners and consultants, demands from general practitioners for X-ray and pathological investigations, and many more complaints, the more I realise how lucky rural practitioners are.

In rural practice the accepted thing both with doctor and patient, and midwife, is that if possible the mother should be delivered at home. This implies a very efficient and searching antenatal set-up, because the essence of the system is that the abnormal pregnancies should be discovered and booked for hospital confinement.

The argument that even normal confinements should take place in hospital because of the danger of sepsis has been proved over and over again to be invalid. Mr. Garland suggests " the lying-in should properly take place at home because after all the patient is not ill ". I would ask him, is the patient ill for the confinement?

So far as our practice is concerned, his argument is that if a woman wishes to stay at home for delivery she should be dissuaded from doing so on hygienic grounds, but if she has been delivered in hospital her wish to stay there for the lying-in should not influence the doctor's decision to send her straight home. The patient's wishes seem to be the last thing Mr. Garland has in mind.

We believe that our present method of basing maternity care on three factors (adequate antenatal care, referral to hospital of abnormalities, and confinement at home of the majority) is the best approach. There would be a danger when the figure for hospital confinements rose to anything approaching 90% or 100% that antenatal care would suffer, since the reason for insisting on this is to be sure of detecting the odd abnormality. Where the mother is in any case to be confined in hospital there would certainly be a tendency to wait until the end, as happened so often in the old days of the " midwife " type of case.

Great Glen,
Leicestershire. G. A. STANTON.

DOCTORS FOR THE FORCES

SIR,—I did not intend to say what ought to be done to form a Combined Forces Medical Service, but only to suggest that it should be planned now while careful consideration and time can be given: for I still have no doubt that we shall have one or suffer the serious lack of it if we survive the onset of a further war.

I gladly concede Dr. Parker (Dec. 9) a closer practical touch with nuclear war: mine is entirely theoretical. However, I wonder how meaningful the distinction is at present. Certainly theoretical considerations give little support to the implications of his letter that nuclear war will occur in conveniently segregated episodes for which a little help from the R.A.M.C. will be sufficient provision.

Oxford. C. W. M. WHITTY.

EXFOLIATIVE GASTRIC CYTOLOGY

SIR,—Dr. Reece and his colleagues (Nov. 25) give their experience of exfoliative gastric cytology compared with radiology in the diagnosis of gastric carcinoma subsequently proved by histology. Some of their points deserve enlargement.

" Schade's technique is easy to perform, taking 10–15 minutes only . . . , yet it has recovered abnormal mucosal cells in 84% of patients with gastric neoplasia ". The reporting of atypical or suspicious cells only confuses physician and surgeon in what is probably already a dilemma. Their 84% of patients with "abnormal mucosal cells " was reached by adding 64% of patients with malignant cells to 20% with " highly atypical " cells. In our experience, which seems to be much the same as theirs, the practical point is that in about a third of all cases of gastric carcinoma ' it is impossible to make the diagnosis dogmatically from cytology. There seems also to be some doubt about the time required. Gastric washings alone are likely to take 15 minutes and after that the fluid must be centrifuged, the smears processed, and the slides examined with care and patience. Surely from start to finish an isolated case takes from 1 to 2 hours?

Both our own experience and the literature leave us in doubt about the reliability of cytology in certain recurring diagnostic problems. Curable gastric carcinoma occurs as peptic ulceration of small patches of neoplastic epithelium, as bleeding or anæmia from ulcerated polyps, as thickening of the pyloric mucosa causing symptoms of mild blockage, and occasionally as a similar process in the neighbourhood of the cardia so that dysphagia results while the growth is still localised. In all these forms endoscopic and radiological diagnosis from similar but benign lesions (which are statistically

1. von Kaulla, K. N., Aikawa, J., Brun, P. *Fertil. Steril.* 1957, **8**, 444.

more probable) is difficult and often impossible; and a recent study of 50 successive 5-year cures showed that mistakes and delay in diagnosis were the rule rather than the exception. In some of these patients a precise diagnosis of malignancy may be expected from exfoliative cytology, positive findings indicating a real usefulness for the method. By contrast, little reliance should be put on negative cytology in excluding carcinoma.

As a general method of screening, or as an additional safeguard against clinical or radiological inexperience, the method has little appeal. It would need at least one or two full-time technicians to examine 1000 patients a year with at best a few extra 5-year cures, and at worst an increased number of unnecessary operations and possibly even fewer 5-year cures.

The London Hospital, London, E.1. D. D. GIBBS
D. JENNINGS.

SEROTHERAPY IN VIRUS DISEASES

SIR,—Dr. Boulter and colleagues (Nov. 4) suggest that, contrary to current teaching, serotherapy may be of value in viral disease even after the onset of signs and symptoms. We recently described [1] an adult with vaccinia gangrenosa, a complication of smallpox vaccination in patients with defective antibody production, who was treated with hyperimmune vaccinial gamma-globulin and blood and survived the illness. Of five patients with this disease reported in the literature and not so treated, none survived. Of eight reported who did receive serotherapy, three survived. In contrast to our patient, there are two other reports of adults with vaccinia gangrenosa, neither of whom received serotherapy, and neither of whom survived.

Though obviously not conclusive, this experience suggests that serotherapy after the onset of disease manifestations is of value in this viral illness, and supports the view that its value in other viral diseases deserves reconsideration.

Upstate Medical Center, Syracuse, N.Y. ROBERT B. ERICHSON.

THE CASUALTY DEPARTMENT

SIR,—I have read with profit the correspondence on this topic, and I should like to answer Dr. Buckley and Mr. Laurence (Dec. 9) in particular.

My reference to the industrial medical service connoted "integration", not a "takeover" bid. I fully appreciate the value of services such as that at Harlow and those of the Gas and Coal Boards and the great industrial concerns.

My proposals affect the casualty side only. The purposes of integration are (*a*) to save dissipation of casualty-officer-hours on minor trauma, and (*b*) to economise in labour-force-hours, now lost in attendance at overloaded and possibly distant hospital casualty departments. The staff to achieve these aims would include whole-time industrial medical officers, hospital casualty officers, and general practitioners. And I think the plan needs Government sponsorship, though private enterprise would still play its part.

The minor injuries in all kinds of workers would pass through conveniently placed first-aid posts. I would have one near my ideal hospital casualty department, and in this hospital first-aid post the apprentice casualty officer would work as part of the general scheme.

The casualty consultant would be an area casualty director; he would control and *train* his juniors in possibly four casualty districts, covering perhaps a million people at risk. Within this framework would be a junior consultant for a large unit, a registrar for a small one. Below this grade would function the appropriate number of house-officers, the apprentice being an essential member of each team. His duties would include the process of discrimination (I dislike the expression "weeding out").

There is no need for the casualty consultant to be called to every difficult case. Once training was successfully established the proficient casualty surgeon would have attained his own special skills: to know the *requirements* of every specialty and to know *when* to seek other opinion. Then he can deflect the patient to the appropriate department. He could of course have some special interests of his own.

The consultant would have casualty beds of his own, to be used in the training of juniors; but cordial cooperation and instruction from all other departmental consultants would be needed in the coordinated training programme.

I see no difficulty here: the surgeon-consultant of the former Highlands and Islands Medical Scheme illustrated the practicability of such tutelage. These men were completely versatile, but I make no claim that they were paragons.

I do not follow Mr. Laurence's statement that poisoning and obscure abdominal injury are seen mainly in provincial hospitals. My introduction to poisoning was in the professorial unit of Glasgow Royal Infirmary (where the main culprit was "red biddie"). There was also a multitude of occult abdominal injuries; and today's increased road accidents have added to the number everywhere.

I have every respect for the orthopædic surgeon but, in the main, his horizon is an osseous one. I would rather have my whitlow treated by a competent casualty officer than by a man skilled in lumbar-disc protrusion. How many orthopædic surgeons would confidently remove a foreign body embedded in the cornea or teach their acolytes how to do it? Or remove a fish-bone from a child's vallecular fossa? Or expeditiously relieve a nasty paraphimosis? And this kind of thing outnumbers broken bones in many a casualty department.

As for the father-figure, in my travels around I have noted that there are proconsuls who, although consultants, are far from consultable. There are those who have never assimilated the art of command. Diatribe and overbearance will never bring serenity or the litigation-fearlessness which I demand from juniors if they are to give of their best in tending the injured. The father consultant must be the image of the summit to be reached, for the juniors would never accept their functions assigned to them if they had not the incentive of further ascent.

If Mr. Laurence will forgive my saying so, the tranquil waters around the lush fields of Hove's county cricket and the factories bear little relevance to the maelstrom of industry's machinations in the North and Midlands. Thuggery is surely unknown down there: it may be revealing for some to know that in certain "black-spots" it has been necessary to employ muscle-men to protect casualty officers from physical assault.

In a permissive society there are, too, many vexatious complaints from disgruntled attenders. These should all be screened by the consultant and not made the subject of direct demoralising inquisition of a junior by administrators. In the more vicious instances, I would have the hospital solicitors give a warning that libellous attacks enjoy no privilege in law, even in the National Health Service. One valiant casualty officer in the North successfully went to court against a local trade body—a refreshing change from the customary letter of propitiation. Casualty officers, to work confidently, must know that they are fairly protected from unreasonable onslaughts from any quarter.

Mr. Pascall (Dec. 9) is right that a well-balanced junior staff is necessary. But, again I say with respect, his view is adorned by the empyrean tors of glorious Devon where, I know, a contented team serves adjacent counties with admirable expertise. But for the infernos of England's industrial hinterland where are we to recruit these young men? Why do we need the palliative measure of £100 inducement payment to 623 of them in difficult areas? I say it is a bribe which serves to bring beginners to the work for a motive that is unworthy: our system should be such that men come to it for the love of it.

I acknowledge the practical wisdom of Dr. Buckley's reference to the sinister figure of Ekonomia stalking my visionary casualty service. My reply is that on the contra-account

1. Erichson, R., McNamara, M. *Ann. intern. Med.* 1961, **55**, 491.

would be the lessened period of hospital care. The workman with a whitlow would be saved from lengthy absence because of a necrotic phalanx; the Health Service bill for a week would be less than for nine; and Cicero reassures us with the paradox that a wonderful revenue lies in thrift!

If economically unable to support an independent medical service, a small factory could combine with a group. I know from discussion with leading industrialists that partial contributions could readily be levied from them. Their profit-graphs would show no decline, if only because of the saving from compensation claims at common law. I endorse wholeheartedly the sentiments of Mr. Ellis and Dr. Claff (Dec. 9).

Towards the end of a long and varied surgical career, I find the greatest inward satisfaction in treating injured people. The pulp of a skilled artisan's forefinger, nicely restored, nowadays gives me greater delight than a pretty cholecystectomy scar. Casualty work, raised to its proper level, will make a generous contribution to the enhancement of human happiness and wellbeing. But the men who are to do it must be cradled and nourished progressively.

Grimsby, Lincs. DANIEL LAMONT.

JAUNDICE AND BILIRUBIN DESTRUCTION

SIR,—In their paper of Aug. 19 on the action of steroid therapy in jaundice, Dr. Williams and Dr. Billing could not account for the drop in serum-bilirubin after steroid administration, since the fæcal urobilinoid was insufficient to explain their finding. They conclude that "steroid therapy must cause or enhance the excretion of bilirubin via an additional metabolic pathway".

Firstly, the findings of Williams and Billing could be accounted for by increased destruction of bilirubin—i.e., breakdown to lower pyrroles not identifiable by known analytical methods—in the intestine. But as the destruction here is due to bacterial action it is hard to see why steroid therapy should influence it.

Secondly, there remains what the authors call "excretion via an additional metabolic pathway"—which so far as I can see means "destruction" in the sense explained above! By "excretion" one usually understands the appearance in an excrete of the substance itself or some readily identifiable metabolite.

The authors do not mention that destruction of bile pigments in this sense probably takes place regularly in horses [1] and cattle.[2] In cattle, occlusion of the common bile-duct and severe lesions of the liver parenchyma do not result in jaundice and give serum-bilirubin values of only a few mg. per 100 ml., while hæmolysis regularly gives values as high as 7–8 mg. per 100 ml. That similar bilirubin "destruction" must occur in occlusive jaundice in man was pointed out by Ottenberg [3]: "What happens to the excess bilirubin in prolonged obstructive jaundice when most of the bilirubin cannot escape from the body is a mystery. Only a small part of the amount formed daily is accounted for by urinary excretion and deposit in the tissues and intestinal excretion . . . Either hæmoglobin . . . must be disposed of by some other mechanism, or the body must have some other way of destroying the excess of bilirubin."

Finally, Watson and others have studied such bilirubin destruction in detail in a young woman with refractory anæmia and normal erythrocyte longevity. Loading with 200 mg. bilirubin daily gave no increase in fæcal urobilinogen or mesobilifuscin, while normal controls responded to such loading with increase of the fæcal urobilinogen. This case is briefly mentioned by Gilbertsen et al.,[4] but during a conversation with me a few years ago C. J. Watson showed lively interest in this little-known pathway of bile-pigment elimination which is so difficult to study quantitatively. What role it may play in the pathogenesis of jaundice in man and other species is hard to say. It may be more important than is generally believed today.

The problem deserves closer study not only in man but also in as many animal species as possible. In my monograph [5 6] on the biology of bile pigments, I referred to this important point, but since Garner's studies no new observations in this line have been published. Cornelius et al.'s recent work [7] on the comparative biochemistry of bilirubin conjugates in various mammal species may, however, stimulate interest in these matters.

Svendborg County Hospital, Denmark. TORBEN K. WITH.

1. Bierthen, E. Inauguraldissertation, Berne, 1906.
2. Garner, R. J. *J. comp. Path.* 1953, **63**, 247; *ibid.* 1955, **65**, 271.
3. Ottenberg, R. *J. Mt Sinai Hosp.* 1943, **9**, 937.
4. Gilbertsen, S., Campbell, M., Watson, C. J. *J. Lab. clin. Med.* 1957, **50**, 818.

PLATELET ECONOMY DURING MODERATE AND INTENSIVE DICOUMAROL THERAPY

SIR,—We much appreciate the interest shown by Dr. Brown and Dr. MacMillan (Nov. 25) in our paper (Oct. 28) and their courtesy in letting us know in advance of their letter and its contents. We feel, however, that their letter calls for some comment.

They seem to impute to us a certainty which we never implied: we have been at some pains to make it clear that this is an idea for consideration rather than a conclusively proved hypothesis. Nowhere in the paper have we drawn from our data the conclusion that low dosage with dicoumarol increases blood coagulability, though we quoted evidence collected by Horlick, by Ashwin, and in experimental animals by our own group which supports such a contention.

Platelet turnover is unfortunately a compound value depending on both platelet half-life and platelet-count. In the value to which they refer, the significant difference is clearly entirely due to difference in the mean platelet-count which (as they so rightly point out) has not been shown to be related to blood coagulability. We are incidentally at something of a loss to see what methods other than statistical can be employed in the analysis of such quantitative data.

It seems reasonable that ultimately anticoagulant treatment should be assessed by its influence on the course of the disease, and effect on survival is probably as good a gauge as any. In their study [8] there were 8 deaths among 27 treated patients and none among 23 controls. Since the numbers were small we evaluated them by Fisher's method of exact probability for two-by-two contingency tables and the value obtained was 0·00414—i.e., the probability of getting such a difference in favour of the control group if the treatment had no effect whatsoever would be about 1 in 240. Of course if it were supposed that treatment is beneficial, the probability would be even more remote. We feel that this evidence is sufficient at least to suspect that treatment according to their regime is harmful. Their study was so carefully designed and executed that we cannot think that any bias can be responsible for the difference. In a further report on this study [9] the results are somewhat better (3 deaths out of 34 in the control group compared with 9 out of 37 in the treated group. This is not statistically significant.) But the death-rate is still if anything higher in their treated group; it is not clear how the periodic analysis of results is to be interpreted since so far as we know there was no provision for optional stoppage in the way the experiment was set up; we understand that there has been some modification in their criteria of control which makes the pooling of data at least hazardous; and finally it must be

5. With, T. K. Biology of Bile Pigments. Copenhagen, 1954.
6. With, T. K. Biologie der Gallenfarbstoffe. Stuttgart, 1960.
7. Cornelius, C. E., Kilgore, W. W., Whear, J. D. *Cornell Vet.* 1960, **50**, 47.
8. MacMillan, R. L., Brown, K. W. G., Watt, D. L. *Canad. med. Ass. J.* 1960, **38**, 567.
9. Brown, K. W. G., MacMillan, R. L., Noble, E. C. *in* Toronto Symposium on Anticoagulants and Fibrinolysins, December, 1961.

recognised that in such a group as this where life expectation is short, deaths alone may be a poor guide since obviously in time all the patients are sure to die and thus the differences between *any* two groups will disappear.

There is considerable difference of opinion about the efficiency of dicoumarol. We have merely suggested a possible explanation for the conflicting results reported. In no sense have we attempted a compelling demonstration, and therefore we cannot be accused of defective logic; but if we may return the compliment we cannot see how "The limited benefit of anticoagulant therapy in any dosage" can be invoked to explain widely discrepant conclusions reached by different investigators.

No doubt there is room for difference of opinion as to the dangers of abrupt cessation of anticoagulation, but we would point out that we made no recommendations whatsoever as to how dicoumarol treatment should be discontinued. Indeed this is a problem to which we have no ready solution. We quite agree that if indeed low doses of dicoumarol are harmful then the practice of tapering doses would have nothing to commend it.

One small remaining point. It would appear from the letter by Dr. Brown and Dr. MacMillan that our statement that low dosage produced changes "in the direction of increased coagulability" referred to all our data. If they refer to the paper again, however, they will see that this statement was made in the paragraph on the in-vitro tests before the results of the in-vivo tests had even been mentioned.

Johns Hopkins Hospital, Baltimore 5, Maryland, U.S.A. EDMOND A. MURPHY.
Sunnybrook Hospital, Toronto 12, Ontario, Canada. J. F. MUSTARD.

NEW PENICILLINS

Mr. G. J. WILKINS, Managing Director, Beecham Research Laboratories Ltd., writes: "Your informative leading article (Dec. 9) includes a reference to an oral penicillin, resistant to staphylococcal penicillinase, which Bristol Laboratories Inc. hope to put on the American market. This compound was discovered in our research laboratories and will be marketed by Bristol Laboratories Inc. under licence from my company. This penicillin is one of a number of compounds, with similar properties, discovered and developed by Beecham Research Laboratories, which are at present undergoing clinical trial in this country. As soon as these trials are complete, details will be made available through the usual channels."

Obituary

GEORGE SECRETAN HAYNES

M.D. Cantab., F.R.C.P.

Dr. G. S. Haynes, consulting physician to Addenbrooke's Hospital, Cambridge, died at Maidstone on Dec. 9, at the age of 89.

He was born in Maidstone and educated at Maidstone Grammar School, King's College, Cambridge, and St. Bartholomew's Hospital. After qualifying in 1897 he held house-appointments in London at the Metropolitan Hospital and the Belgrave Hospital for Children before beginning his long association with Addenbrooke's Hospital as house-physician in 1898—a post which he held for four years. After a short time as resident clinical assistant at St. Luke's Hospital in London, he returned to Cambridge and settled in general practice. In 1905 he was appointed honorary anæsthetist at Addenbrooke's and also worked in the university pharmacological laboratory. He published several papers on the standardisation and actions of drugs of the digitalis group, on the salicylates, and on the treatment of asthma and heart-failure. In the 1914-18 war he was mobilised as a captain in the R.A.M.C. (T.F.) with the 1st Eastern General Hospital at Cambridge and later moved to Wimereux with the 55th General Hospital, attaining the rank of brevet major. In 1919 he was appointed honorary physician to Addenbrooke's Hospital, and he was elected F.R.C.P. in 1932. He was president of the Cambridge Medical Society in 1926, and again in 1949, and he examined for many years in the final Cambridge M.B. He retired in 1937, but he sat on medical recruiting boards throughout the years of war and conscription until his health began to fail. After the death of his wife in 1955 he returned to his home town, Maidstone.

C. H. B. and L. C. M. write:

"Haynes was a genial and lovable man, affectionately known as Pop by his family and many generations of house-physicians, examiners, and undergraduates. He was a shrewd general physician, and his equanimity and kindliness won the confidence and affection of his patients of all ages. He was a meticulous prescriber of medicines, and habitually carried a little book carefully indexed under symptoms and diagnoses which contained a wide variety of prescriptions and proprietary remedies. His affection for Addenbrooke's was tremendous, and he strongly encouraged the early steps towards a liaison between the hospital and university. He delighted in the company of his friends, and the Corner House on Trumpington Street was a centre of hospitality."

Dr. Haynes is survived by two sons, one of whom is a doctor, and twin daughters.

ARTHUR MAX BARRETT

M.D.Cantab.

Dr. Max Barrett, morbid anatomist in the University of Cambridge and histologist to Addenbrooke's Hospital, died on Dec. 11 at the age of 52 years.

He was born at Thaxted in Essex, where he spent his boyhood. He was educated at Newport Grammar School and at the Cambridge and County High School, where he won a State scholarship in 1927 and a major scholarship to Pembroke College, Cambridge, in 1928. He obtained first-class honours in part I and part II of the Natural Sciences Tripos and was awarded the Shoolbred and Foundress' scholarships. He went to the London Hospital Medical College as an entrance scholar in pathology and won five prizes during his clinical training. After taking the Cambridge M.B. in 1934, he gained wide experience in medicine and pathology at the London Hospital as house-physician to the medical unit, pathological assistant in the Bernhard Baron Institute, and Sir Hailey Stewart research fellow.

Dr. Barrett was acting as temporary assistant director of the Hale Clinical Laboratory when in 1938 he was invited to return to Cambridge as a university demonstrator in pathology. During the war years he was the only demonstrator in the department and shouldered a large part of the teaching responsibility. Despite this he found time to take an active interest in the routine pathology services of Addenbrooke's Hospital, which were at that time isolated from the university. His patient work laid the foundations of a close and fruitful union between the hospital and the university which led to the complete fusion of their pathological departments in 1946. In that year Barrett was appointed to the newly created post of university morbid anatomist and histologist to Addenbrooke's Hospital. His thesis for the M.D. degree, on measurements of arteries and the postmortem interpretation of cardiomegaly, embodied the results of eleven years' work, and won the Raymond Horton-Smith prize in 1960.

J. H. R. writes:

"Although his basic training and experience in pathology were very broad, Barrett excelled in morbid anatomy and

histology. His earliest interest was in fundamentals; he investigated fully the technical methods that he used, adding contributions to many of them including the Paul-Bunnell test, bone-marrow staining, and removal of formalin pigment. Later he investigated with wholehearted enthusiasm and painstaking care everything that came his way, exercising equal care whether the matter seemed important or trivial. Since he was blessed with an accurate memory his knowledge rapidly became both deep and wide, and his opinion and advice were greatly valued. By applying patient and keen observation and undivided attention to a problem he could shed light on it with such apparent ease that the casual observer was unaware that any problem had ever existed. He knew full well that an answer cannot be found to every problem; when he was unable to provide a definite diagnosis he was careful to say so, usually going on to indicate in meticulously chosen words the cause and extent of his uncertainty.

"He was master of every aspect of his subject; but it pleased him especially to find out about blood diseases and the pathology of the locomotor system, the pituitary, and the kidney. He was also interested in the problem of sudden death in infancy, but above all he was fascinated by arteries, a subject to which he had been introduced by H. M. Turnbull. His publications were worthy though few. This he did not regret, preferring to impart knowledge to individuals. To postgraduate students particularly, he was a great teacher because he showed them how to think for themselves. He always made clear the logical steps by which he had reached his conclusions. A procession of young pathologists owe their interest in morbid anatomy to his teaching.

"He was satisfied only by the best; by his example he encouraged his students, assistants, and colleagues to do good work; he rarely issued orders. He would not allow standards to be lowered for any reason whatever: expediency was never considered. Although his high standards and shy manner proved a little frightening to newcomers to his department, they were soon led to appreciate his great warmth of heart and the unshakeable loyalty with which he supported them in all their endeavours. He had a sympathetic understanding of people and yet his appraisal of their qualities was dispassionate and exact. This quality, together with an obstinate determination and a strong sense of justice, made him an ideal committee chairman when the subject under discussion was controversial. He believed in giving young people a chance to show their ability by delegating relatively important work to them. When he did this he would restrain himself from interfering, even when much tempted to do so, and if the results did not come up to his standards he accepted complete responsibility. The strongest rebuke he would ever deliver began 'It would have been better if . . . '.

"Although he put so much time and energy into his work, his life was very rich in other ways. He had a deep understanding of music and served as secretary to the Cambridge Philharmonic Society for about twenty years, being especially enthusiastic about their more adventurous programmes. He was a keen naturalist with a special interest in fungi, which he studied extensively both in the field and by histological techniques. He had a large and united family in which he took great pride and which was a great source of happiness to him.

"Those of us who worked for him will not forget A. M. B. with his slightly untidy appearance and his unassuming but purposeful manner. We shall recall the characteristic pause for reflection before he replied to even the most trivial remark. But above all we shall remember him as a good man whose patient guidance put us straight time after time, however stupid or ignorant we might be."

Mr. G. R. EDWARDS, who died on Dec. 10 at the age of 70, was secretary of the Royal Society of Medicine from 1925 to 1951. He was also for many years editor of the *Catholic Medical Guardian*. He was appointed O.B.E. in 1949, and when he retired from the secretaryship he was made an honorary fellow of the Royal Society of Medicine.

Notes and News

HOSPITAL MATERNITY CASES

INFORMATION on the cause of hospital admission and duration of stay of maternity patients is contained in part II [1] of the report for 1958 of the hospital inpatient inquiry. This inquiry is organised jointly by the Ministry of Health and the General Register Office. Hospitals of all types, except those solely for psychiatric patients, in England and Wales provide a confidential short summary of the case-record of one in ten of all patients discharged (or dead).

It was estimated that 579,400 patients were discharged in 1958 after care in hospital for pregnancy and childbirth (the estimated total in 1956 was 538,500). Mothers stayed an average of 9·6 days after the birth of the baby in 1958, compared with 10·4 days in 1956 and 11·0 days in 1955. Only 45·7% of the admissions for maternity care were "completely normal" maternity cases in the sense that they did not need inpatient antenatal care and had a normal delivery and puerperium with no complications at any stage. 10% of the admissions were for antenatal care only, and 15·5% for antenatal care followed by delivery; 62·7% were admitted in labour; 2·3% were admitted for postnatal care after delivery elsewhere; and 9·5% were admitted for miscellaneous reasons including abortion. Toxæmia of pregnancy was present in 33·4% of the patients who received antenatal care only, and in 42·2% of those with antenatal care followed by delivery.

HOSPITAL COSTS

THE three parts of the *National Health Service: Hospital Costing Returns*[2] have now been published, and together they cover the 460 hospitals in England and Wales which have used the detailed system of costing during 1960-61. The individual returns of the remaining 2000 hospitals, which are using a less detailed system, will not be published (though they are available within the service), but they are included in the national averages given in part I. According to these, the weekly costs per patient increased during the year by rather more than 8%, of which 6% was due to increased salaries and wages, reduction of nurses' hours, and rising prices. The cost per case treated rose by 6%.

For non-teaching hospitals the average cost per inpatient week was £27 13*s.* 6*d.* for 1960-61 (compared with £25 12*s.* 0*d.* for 1959-60) and per inpatient case £53 19*s.* 9*d.* (£51 1*s.* 1*d.*). Comparable figures for London teaching hospitals were £36 7*s.* 4*d.* (£39 6*s.* 8*d.*) and £89 4*s.* 7*d.* (£82 13*s.* 5*d.*).

Part II gives detailed costs for seven lay departments (such as administration and catering), and part III for four medical departments (radiotherapy, diagnostic radiology, pathology, and physiotherapy).

APPROVED NAMES FOR DRUGS

The British Pharmacopœia Commission has issued the following new supplementary list of approved names:

Approved name	*Other names*
Aloxiprin	Polymeric condensation product of aluminium oxide and *O*-acetylsalicylic acid Palaprin
Carbinoxamine	*p*-Chloro-α-2-pyridylbenzyl 2-dimethylaminoethyl ether Clistin is the maleate
Dichloralphenazone	A complex of chloral hydrate and phenazone Welldorm
Dydrogesterone	9β:10α-Pregna-4:6-diene-3:20-dione 6-Dehydro-9β:10α-progesterone Duphaston
Etryptamine	3-2'-Aminobutylindole α-Ethyltryptamine Monase is the acetate —
Fluocinolone	6α9α-Difluoro-11β:16α:17α:21-tetrahydroxypregna-1:4-diene-3:20-dione 6α:9α-Difluoro-16α-hydroxyprednisolone Synalar is the acetonide
Methyl Cysteine	Methyl α-amino-β-mercaptopropionate Acdrile is the hydrochloride
Pentalamide	*o*-Pentyloxybenzamide *O*-Pentylsalicylamide

1. Report on Hospital Inpatient Enquiry for the Year 1958. Part II: Detailed Tables and Commentary. H.M. Stationery Office. Pp. 301. 17*s.* 6*d.*
2. National Health Service: Hospital Costing Returns, year ended March 31, 1960. Parts I, II, and III, 1 guinea each. H.M. Stationery Office.

Phenaglycodol	2-*p*-Chlorophenyl-3-methylbutane-2:3-diol Ultran
Phenbenicillin	α-Phenoxybenzylpenicillin Penspek is the potassium salt
Sodium Metrizoate	3-Acetamido-2:4:6-tri-iodo-5-*N*-methylacetamidobenzoate
Thiethylperazine	2-Ethylthio-10-[3-(4-piperazin-1-yl)propyl]-phenothiazine The name Torecan is applied to the base and its salts

Proprietary name	*Approved name*
Acdrile	Methyl Cysteine
Clistin	Carbinoxamine
Duphaston	Dydrogesterone
Monase	Etryptamine
Palaprin	Aloxiprin
Penspek	Phenbenicillin
Synalar	Fluocinolone
Torecan	Thiethylperazine
Ultran	Phenaglycodol
Welldorm	Dichloralphenazone

University of Liverpool

Dr. Thomas Wilson, senior lecturer in tropical hygiene in the Liverpool School of Tropical Medicine, has been appointed to the Middlemass Hunt chair of tropical hygiene in succession to Prof. T. H. Davey, who has retired.

Dr. Wilson, who is 56 years of age, was educated at Queen's University, Belfast, where he graduated M.B. in 1927. After holding resident posts at the Belfast City Hospital, he joined the staff of the central health board of the Federated Malay States in 1929. The following year he took the D.T.M. & H. and was awarded the Milne medal. He was appointed health officer with the Malayan Medical Service in 1931. During the late war he served with the F.M.S. Volunteer Force, and later as a captain in the R.A.M.C. He spent some years as a prisoner-of-war in Malaya and Thailand. On demobilisation in 1946 he returned to the Malayan Medical Service, and in 1949 he joined the Institute of Medical Research in Kuala Lumpur as a senior malaria research officer. He was appointed acting director in 1953 and director in 1956. He was appointed C.B.E. in 1959, when he left Malaya to take up his present post. He has served on study groups and committees of the World Health Organisation and the Colonial Office, and as secretary of subcommittees of the Colonial Medical Research Council and of the Medical Research Council. His published work includes papers on malaria and filariasis.

University of Edinburgh

Mr. J. F. Cullen has been appointed senior lecturer in ophthalmology, and Mr. P. W. Harvey and Mr. R. M. Preshaw assistant lecturers in clinical surgery.

University of Dublin

On Dec. 11 the following degrees were conferred:

M.D.—Mavis Archer, A. M. Cantor, John Higginson.
M.A.O.—F. A. Best.
M.B., B.Ch., B.A.O.—Andrew Bhagwansingh, J. E. T. Byrne, Dimitri Leonidas Contostavlos, Teresa A. Farbotko, G. R. Henry, P. E. Lawton, Cintra Savitri Moosai-Maharaj, J. V. O'Brien, Owen O'Connor, G. B. Robinson, A. K. Spence, Marjorie J. Wallace.

Royal College of Surgeons of Edinburgh

At a meeting of the college on Dec. 8, with Prof. John Bruce, the president, in the chair, Martin Fallon was admitted to the fellowship without examination. The following were admitted to the fellowship:

Mohamed Ibrahim Abdalla, Fateh Khan Akhtar, D. St. C. Baird, E. B. Gonzalez, Birendra Nath Chakrabarti, K. V. Cooper, Pradipta Kumar Das Gupta, W. J. de Vries, J. McP. Drummond, Vinod Kumar Dubey, Mohamed Kamal El-Din Hussein El-Rifi, W. H. H. Garvie, Somnath Jaivant Gude, Ranjit Kumar Guhathakurta, S. C. Hossy, Diana Suan-Liew Ip, Bennett Arumabadhu Jayaweera, Subramaniam Kalyanaraman, Brij Mohan Lal Kapur, P. G. Keogan, Puzhankarai Krishna Menon, Soli Jamshed Lam, Maung Maung Lay, Lodewyk Arnoldus Slabbert Lemmer, Shankar Hari Limaye, I. V. Lishman, Thomas Lo, Ratna Prasad Lohani, A. G. MacIntyre, O. H. A. Mitchell, Arabinda Mitra, Sib Das Mukhopadhyay, S. W. Parkinson, Inder Perkash, D. M. Phillips, Thomas Pirotta, Ajay Kanti Poddar, R. H. Pont, Shiu Chandra Ramrakha, B. D. Shepherd, V. H. Smith, A. W. Swanton, Abdel Khalik Moustafa Terzaki, L. A. Watson, J. G. Whitaker, W. L. Whitehouse, B. J. Wilken, J. H. Youngleson, Mohemed Diaa El Din El Zawahry.

Ciba Foundation Lecture on Research on Ageing

Dr. R. J. van Zonneveld of the Nederlands Instituut voor Praeventieve Geneeskunde, Leiden, will give this lecture on Friday, Jan. 19, at 5 P.M., at the General Infirmary at Leeds. He will speak on the national survey of the health of the aged in the Netherlands.

Daphne Somerset Fry Memorial Trust

This trust has been started by the husband of a patient who died of Bright's disease. The University of Oxford has agreed in principle to accept from the trust an endowment to establish a chair of medical research, with special reference to Bright's disease and cognate problems. At least £120,000 is needed. The trustees include Sir George Pickering and Lord Cohen of Birkenhead. Further particulars may be had from the secretary of the trust, 52, Beauchamp Place, Knightsbridge, London, S.W.3.

Cancer Registration

The Ministry of Health has asked that all hospitals which are not yet participating in the national cancer registration scheme, or which do not yet provide a complete register of all cancer cases, shall do so (H.M. [61] 108). The Minister is to review the position at the end of next June, when he hopes to find that registration is complete.

Inquiry into Radiation Doses

Accidental overdoses of superficial X-rays given last month to 12 patients at a Plymouth hospital are to be the subject of an inquiry by a committee (*Times*, Dec. 14).

Industrial Health Advisory Committee

This committee has been reconstituted, and the medical members are Dr. K. P. Duncan, Prof. R. E. Lane, Dr. J. A. L. Vaughan-Jones, Dr. L. G. Norman, Prof. R. S. F. Schilling, Dr. Roland Nightingale, and Prof. R. C. Browne.

Dr. Walter Pagel has been awarded the honorary degree of M.D. of the University of Basle in recognition of his study of Paracelsus.

Applications for an Eli Lilly medical research fellowship ($300 a month for twelve months and travelling expenses) are invited from doctors registered in South Africa who wish to undertake research in the United States. Further details can be obtained from Dr. H. A. Shapiro, P.O. Box 1010, Johannesburg. The closing date for applications is Feb. 28.

CORRIGENDUM: *Chemical Tests for Phæochromocytoma.*—In the letter (Dec. 9) by Dr. Kelleher and Dr. Walters we unfortunately introduced errors into the reference numbers in the text. On p. 1312, 5 and 6 in the text should be 17 and 18; and on p. 1313, 23 in the text should be 24.

Appointments

CATTON, BESSIE J., M.B. Edin.: assistant M.O. and school M.O., Castleford division, West Riding of Yorkshire.
LAWSON, I. R., M.D. Aberd., M.R.C.P.E.: consultant physician, North-Eastern Regional Hospital Board geriatric service, Scotland.
LYALL, M. G., M.B. Aberd., D.O.: assistant ophthalmologist, Aberdeen teaching hospitals.
MCGILL, D. A. F., M.D. Lond., M.R.C.P.: consultant physician, Winchester and Alton hospital groups.
RASHBASS, BARBARA J., M.B. Lond., D.C.H.: assistant M.O., Middlesex (Wembley and Willesden).

Lancashire County Council:

CARROLL, MARY S. A., M.B. N.U.I.: assistant divisional M.O., Worsley.
MCMURDO, LYDIA, M.R.C.S., D.P.H.: assistant divisional M.O., Urmston.
POWER, ROSALINE M., M.B. N.U.I., D.C.H., D.P.H.: assistant divisional M.O., Kirkby.
SEYMOUR, MARGARET W., M.B. Manc.: assistant divisional M.O., Nelson.

Liverpool Regional Hospital Board:

COHEN, R. I., M.B. Lpool, F.R.C.S.: consultant general surgeon, Victoria Central Hospital, Wallasey.
DALBY, J. E., M.B. Cantab., F.F.R., D.M.R.T.: consultant radiotherapist, regional radiotherapy service.
GLAZEBROOK, G. A. L.-G., M.B. Lond., M.R.C.P.E., F.F.R., D.M.R.T.: consultant radiotherapist, regional radiotherapy service.
KUPFER, E. M., M.B. Lond., F.R.C.S.: consultant orthopædic surgeon, Chester hospital group.
LEVITT, MENDEL, M.B. Cape Town, D.A.: consultant anæsthetist, Chester and North Wirral areas.
MIDDLEFELL, ROBERT, M.B. Lpool, D.P.M.: consultant psychiatrist, Deva and Moston hospitals.
PRITCHARD, A. E., M.CH.ORTH. Lpool, F.R.C.S.E.: consultant orthopædic surgeon, Warrington hospital group.
RICHARDSON, J. D., M.D. Lond., D.P.M.: consultant psychiatrist, Deva and Moston hospitals.
STEWART, M. A., M.B. N.Z., M.R.C.P.E., D.M.R.: consultant radiotherapist, regional radiotherapy service.
THOMPSON, W. A. L., M.CH.ORTH. Lpool, F.R.C.S.E.: consultant orthopædic surgeon, Walton and Fazakerley hospitals.
THORNBERRY, C. J., L.R.C.P.I., M.R.C.O.G.: consultant obstetrician and gynæcologist, North Wirral and Birkenhead hospital groups.
WETHERELL, G. A., M.CH.ORTH. Lpool, F.R.C.S.: consultant orthopædic surgeon, Clatterbridge Hospital.